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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D. Lond.

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SYNOPSIS.

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—*Treatment by Blisters.*—Those cases answer best to the treatment by blisters, freely placed over the inflamed joints, in which a great number of joints are simultaneously affected, and when by setting up a large amount of discharging surface in the proximity of the inflamed parts, a large proportion of the materies morbi may be evacuated at one *coup*. The urine diminishes in acidity during this treatment, and in 10 cases out of 43 became positively alkaline. (Dr. Davies, p. 36.)

FEVER.—Let the aliment chiefly consist of milk and water, butter-milk, rice and milk, arrow root, and beef-tea, sometimes with a little bread, where it is relished. This must in great measure be left to the patient's own feelings, but milk diet should in all instances form the staple nourishment. Neither any medicine nor alcoholic stimulants should be given as matter of routine, they should be directed entirely to emergencies and casualties. (Dr. W. T. Gairdner, p. 9.)

The use of stimulants in disease is not now so much in vogue in many of the London Hospitals as was formerly the case. In the treatment of typhus and typhoid fevers, although a little wine may be very useful occasionally, it is rarely necessary, for alcohol be it remembered is not an antagonist to the fever. (Dr. Wilks, p. 1.)

GOUT.—There are two ways in which an excess of uric acid may occur in the blood and textures: 1st, from an excess of animal or vegetable albuminous food entering the system; and 2nd, from an arrest of oxidation. It is evident therefore that there are two modes of preventing the gouty diathesis: first, by diminishing the amount of albuminous food; and secondly, by promoting oxidation. In other words, the smallest amount of food, and the greatest amount of air are the antidotes for the gouty diathesis. (Dr. Bence Jones, p. 109.)

Chronic Gout treated by dilute Hydrochloric Acid.—It is better to direct our treatment in cases of chronic gout to increase the efficiency of the assimilative process, and so prevent the formation of lithic acid, than simply to aid its elimination by the administration of alkalies after it is formed. Hydrochloric acid with cascarilla is more likely to be useful than potash and lithia water. Several interesting cases are related, in which most marked success followed the judicious carrying out of this plan of treatment. (Dr. James F. Duncan, *Dublin Quarterly Journal, May* 1865, p. 302.)

Malarious Fever.—Quinine may be given in scruple doses whether there are head symptoms, delirium, coma, or pain in the liver. Whether it be in the hot stage or cold, quinine is not only safe for all forms of malarious fever, but its certain cure; and in cases where there is danger to life, the earlier and the larger the doses of quinine which can be given to the patient the better. Vomiting and full cinchonism should be the only checks for continuing it in full doses. (Mr. E. Hare, p. 24.)

The action of quinine when administered in large full doses, say of half a drachm followed by a scruple every two hours, is that of a powerful nervine sedative. It has almost a paralysing action on the brain. In some cases even dangerous symptoms are produced: deadly coldness, arrest of the pulse, and paralysis of the heart. In smaller doses it produces the excitement usually spoken of. In sufficient doses it is however a specific, and with water to wash out the bowels, and apply to the head, no other medicine is wanted for the successful treatment of the worst forms of tropical fever. It is well always to mix calomel with the quinine when the patient is a child; its effects are marvellous in these cases, even when no other visible effects are produced. Five grains of quinine is a sufficient dose for a child, but without calomel double this dose is necessary. (Mr. E. Hare, p. 28.)

Rheumatic Fever.—*Alkaline Baths.*—No local application will relieve the pain so much as spongio-piline soaked in a warm saturated solution of nitrate and carbonate of potash, and applied to the joints. By the use of an alkaline bath in which the whole body may be immersed, an attack of rheumatic fever may be arrested in limine. The bath should contain about twenty-four gallons of water at 100°, two pounds of bicarbonate of potash, and one pound of nitrate of potash. The patient should remain in it at least ten minutes. (Mr. B. G. Everett, p. 40.)

SCARLATINA.—*Bisulphite of Soda.*—The sulphites, so much recommended in cases of catalytic disease, as scarlet fever, act as preventives if taken when the person is exposed to infection: but when the catalytic action has once taken place, as evidenced by the presence of the rash, no sensible effects are produced by their administration. In severe cases the salt should nevertheless be administered in doses of ten grains every one, two, or three hours; for the chief danger is the tendency to secondary inoculation of the blood which the sulphurous acid, set free when a bi-sulphite is decomposed by the acid of the stomach, tends to prevent. It may not only be used in scarlet fever but all infectious febrile diseases. (Dr. W. J. Cummins, p. 34.)

TYPHUS FEVER.—In a large proportion of cases, typhus fever left to its natural course, and treated with abundant milk diet and without drugs or stimulants, will have its natural crisis before the twelfth day. No other food can be depended on than milk, or buttermilk; to give wine, whisky, and beef tea while withholding milk is to diminish the chance of the patient's recovery. Large doses of alcoholic stimulants are positively injurious, as is also the deprivation of a plentiful supply of fresh air. Our guide to the progress of a case of fever is, to watch carefully and constantly the rate of the pulse, for it gradually rises in frequency up to the acme of the fever, and then as gradually declines, so that the crisis can be accurately ascertained. This is of immense importance as regards the prognosis, for the other symptoms may continue as formidable as ever, but if the pulse is gradually declining in frequency, the case will do well. (Dr. W. T. Gairdner, p. 7.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CHOREA.—*Aniline.*—Sulphate of aniline has a powerful sedative influence on the nervous system when taken internally. When taken in medicinal doses, say of two to three grains thrice a day, it is an excellent remedy in cases of chorea, in some cases rapidly diminishing the muscular irritability. After a time it produces blueness of the lips and face; this, however, is only temporary. It is also liable to produce great depression. Its use must then be left off for a short time. Aniline is a distinct thing from nitro-benzine, which is a product similar to glonoine (produced by the action of nitric acid on glycerine) and to gun cotton (produced by the action of nitric acid on cotton). Nitro-benzine has very noxious properties. Of the metallic tonics iron is the best, arsenic next. Cod-liver oil is frequently of service, by improving the

kidney into the bladder, the kidneys being in their general structure healthy. The lithotrite should under no circumstance be used for the purpose of crushing an oxalate of lime calculus, the character of which is generally recognised by its sharp ringing sound. The fragments are always sharp and irritating to the bladder, and it not uncommonly happens that the lithotrite fails to act on the great mass of the stone. (Mr. Holmes Coote, p. 231.)

Oxalic Acid Diathesis—The best medicinal treatment of the oxalic acid diathesis is that recommended by Prout, viz., the use of mineral acids. It is better to use each acid separately, according to the properties which each possesses, than to use the nitro-muriatic acid in one invariable form, as is generally done. As hydrochloric acid is especially the acid of the gastric juice, it should be preferred where there is feeble digestive power, and given before each meal of animal food. Nitric acid promotes secretion and perhaps oxidation, and it should be given an hour or two before meals on an empty stomach, in cases where secretion is defective. (Dr. H. Bence Jones, p. 125.)

Phosphatic Gravel and Calculi.—These may arise from four causes, all requiring different treatment. When there is retention of urine the cause of this must be removed before anything else can be done. When the stone is not due to any vice in the general system, but to a purely local cause, as where phosphates augment a small uric acid calculus, all tendencies to the uric acid deposit having long since passed away, then the stone itself must be attacked. Sometimes there is an excessive secretion of phosphates from the kidneys, in other words, a genuine phosphatic diathesis: here the vice in the tissues must be got rid of. Lastly, there may be simply a deficiency of the normal acidity which is essential to the retention of the earthy phosphates in solution until its emission from the bladder. Of all calculi the phosphatic are the most soluble, hence it is better worth while to try to procure solution of the stone in this than in the other varieties of stone. (Dr. G. Harley, p. 136.)

Renal and Urinary Affections.—*Tincture of Perchloride of Iron.*—The tincture of iron possesses most eminently the properties of an astringent, tonic, and styptic, coagulating blood or albumen with which it is brought into contact, and constringing the vessels and tissues to which it is applied. Hence arises its value in the albuminuria of Bright's disease, and in hemorrhage from the kidney, bladder, or urethra. We can only conclude, however, that it acts by its tonic

influence on the system, for by analysis it may be proved that neither the iron nor the acid escape by the urine. (Dr. A. H. Hassall, p. 150.)

AFFECTIONS OF THE BONES, JOINTS, ETC.

Exposure of Articular Cartilages in Wounds.—It is generally believed that the exposure of articular cartilages in wounds is dangerous, this cartilage being liable to inflame and prevent the healing of the parts. This is entirely incorrect. Entire bones of the tarsus may be removed leaving a wound lined with articular cartilage, and yet no bad symptoms occur, and the wound will heal soundly, though of course rather slowly, owing to the nature of the wound. It may be reasonably alleged as probable that a wound in which no bony tissue is exposed is more likely to heal kindly and soundly, and is less liable to pyæmic infection than one where a large surface of cancellous bone is laid bare, with all its cells cut open. Such bone frequently softens and becomes carious, and has to exfoliate; and if this is the case with healthy bone, when exposed and divided in operation, it is still more often the case with bone already softened by low inflammation. (Mr. T. Holmes, p. 176.)

Fracture.—*The Immediate Treatment of.*—Out of ninety of the principal surgical hospitals of Great Britain and Ireland, in seven only is the custom of early putting-up of the fracture by starch or other fixed material resorted to. This is no doubt owing to the constriction of the limb produced by the starch bandage as ordinarily employed. This, however, may be avoided by making the starched linen splints in two halves. Let a piece of wet lint be accurately adapted to the outer side of the leg, and over this a series of pieces of bandages diagonally, reaching from the inner edge of the tibia to the inner edge of the calf, or as far as can be reached, the patient lying quiet with the outer side of the limb uppermost. These pieces of bandage must be smeared on their inner surface with a mixture composed of the whites of eight eggs and half a pound of flour. The instep must be accurately covered, also the toes, sole, and heel, not a wrinkle or crease being left. The limb must be left uncovered until next day, by which time the half splint will have become dry and extremely firm and light. The patient must now turn upon the other side and expose the inner side of the limb, and the proceeding must be repeated, care being taken that the lint lining is in advance everywhere of the plastered strips, to prevent adhesion of the two halves. When the second half has dried the

edges of the two may be neatly trimmed with scissors, the edges however being allowed to overlap half an inch or so. A roller bandage must now be put on and the splint is complete. Subsequently, when the patients come to exercise and move about, and the size of the limb begins to decrease, chamois leather may be moulded to the interior to produce a softer lining. (Mr. J. Tufnell, p. 163.)

Penetrating Wounds of the Knee-joint. — *Irrigation.* — In cases of penetrating wound of the knee-joint, the parts should be kept immovable by being fixed to a back splint sufficiently long, and well padded. The bandage should not cover the joint itself. A continuous dripping of water should now be directed on the part, by means of a few threads of worsted, one end of which is placed in a vessel of water placed three or four feet above the limb. Nothing can be compared to this plan for preventing inflammation of the joint. It may be kept up seven or eight days, and this period may be extended or shortened according to the special requirements of each case. (Dr. W. Newman, p. 177.)

Stiff Joints.—*Forcible Extension under Chloroform.*—The chloroform must be given so as to produce complete muscular relaxation, and then, supposing the knee-joint is to be operated upon, let the operator flex the joint somewhat more than it already is flexed, whilst the thigh is held firmly by an assistant. This should be done by a sudden sharp jerk. The adhesions usually give way with a loud noise. The joint must then be forcibly straightened, and if this cannot be accomplished perfectly, a few more sudden jerks may be given, and the extension repeated until the limb is perfectly straight. If, however, it is found that the force to be used is very considerable, it is better not to do too much at one operation. The joint should be firmly bandaged after the operation, and the limb fixed in a good position on a splint. If swelling and heat follow, hot fomentations are the best treatment, but in a large number of cases even this is not required. Strumous cases are decidedly the most unfavourable ones, and a stiff joint is certainly the best result that can be expected. (Mr. W. Adams, p. 186.)

AFFECTIONS OF THE SKIN.

Eczema.—The treatment of eczema resolves itself into the treatment of constitutional and local debility. Arsenic is a remedy having a peculiar power in cases of eczema. It is a tonic which gives force to and improves the nutrition of the nervous matter, and accelerates the actions of the skin, which

tend to its nutrition and renovation. It should be combined with iron, and given in small quantities in the middle of each meal. So long as there is acute inflammation, water dressing should be applied. Subsequently, all crusts having been removed, the part should be dressed with strips of lint thickly spread with benzoated ointment of oxide of zinc, which may be kept in place by straps of plaster, or a cotton bandage very evenly applied. Every forty-eight hours it should be unpacked, re-dressed, wiped with a soft napkin to remove crusts, and packed up again for a similar period. If there is much heat, the bandage may be sopped with water. If there is much irritability the part should be undressed and washed thoroughly with juniper tar soap, then dried and again dressed as before. At other times pencilling the surface with a weak solution of nitrate of silver in nitric ether is of more service. The chronic forms of eczema owe their thickening and induration to infiltration of the tissues of the skin with serous fluid. This state may be rapidly cured by brushing the part over with caustic potash. (Mr. Erasmus Wilson, p. 259.)

Bichloride of Mercury and Antimony.—Although eczema mostly arises in weak states of constitution, it is in its own essence inflammatory, and therefore, whilst the strength is supported by generous diet, an alterative remedy like bichloride of mercury may be most advantageously given, and even combined with tonics. Antimony is also very useful in some cases; it may be given in combination with the mercury, or, in cases where the skin is heated and inclined to be dry, along with salines. The combination of mercury and antimony will sometimes cure in a week or two, cases of eczema in oldish people in the parts about the anus, which cases frequently will have resisted other treatment for years. One-sixteenth of a grain of bichloride given three times a day with one-eighth of a grain of antimony is a sufficient dose. To infants one-sixtieth of a grain of bichloride should be given three times a day, with a little glycerine, which is a very good demulcent. If given for some months, with occasional intermissions, it will mostly cure the complaint. Beef-tea and broth should also be given in addition to the milk, and farinaceous food avoided. External applications must of course be combined with this treatment. (Mr. R. H. Meade, p. 268.)

Eczema of the Eyelids, Conjunctiva, and Cornea.—What is known as ophthalmia tarsi is in reality eczema of the lids, and is frequently associated with eruptions on the skin. Granular lids, lippitudo, strumous ophthalmia, certain forms of simple or catarrhal ophthalmia, keratitis, and strumous keratitis,

and certain ulcers on the cornea, are merely varieties of eczematous disease. Unchecked eczema of the eyelids always terminates in lippitudo. The treatment should be directed to eczema, its chief features being a non-stimulating diet and alkaline medicines, with the local use of eczema ointments, and the administration of morphia if there is much photophobia. (Mr. Furneaux Jordan, p. 269.)

Parasitic Skin Diseases.—*Carbolic Acid.*—Carbolic acid is one of the most efficient parasiticides known. It must be used as a lotion after softening the part with warm water. (Dr. Watson, *British and Foreign Medico-Chirurgical Review*, *Jan.* 1865, p. 240.)

Profuse Perspiration.—*Hot Water.*—The application of hot-water to the skin, that is, of water as hot as can be borne without pain, instead of leaving the skin moist leaves it hot and dry. Hence it may be used as a remedy in cases of profuse perspiration. It may be useful to those who though otherwise in good health perspire to a distressing degree in hot weather; also in cases of perspiration of some particular part, as the axillæ, hands, feet, &c. The health in these cases is sure to be improved by free purgation, and by quinine, fresh air, and exercise. In the ordinary night sweat of phthisis it is productive of great relief; but in the true hectic, arising from absorption of decaying pus from some internal organ, it must be confessed to be of no avail. (Dr. R. Druitt, p. 71.)

Scabies.—Boil one part of quick-lime with two parts of sublimed sulphur, in ten parts of water, until the lime and sulphur are perfectly united; during the boiling it must be constantly stirred with a piece of wood, and when the sulphur and lime have combined the fluid is to be decanted, and kept in a well-stoppered bottle. It is to be used for the cure of scabies as follows:—Rub every part with it, except the face and scalp, for half an hour, then wash well and put on clean linen. A single application will suffice. The linen must also be soaked in boiling water, and the clothes subjected to a temperature of about 200° Fahr., in an oven for a short time. (Dr. J. F. Nicholls, Mr. T. M. Kendall, pp. 274, 276.)

Tincture of Iodine.—The compound tincture of iodine is the cleanest and surest cure for scabies, when the surface affected is not very extensive. It should be applied once a day with a camel's-hair brush. At the first application the clusters of spots may be painted together, but afterwards it will be necessary merely to touch any fresh ones which may appear. (Dr. J. Lowe, p. 275.)

Oil of Petroleum.—The oil of petroleum instantaneously kills the parasite, while it is, at the same time, a disinfectant against the larvæ which are wont to be found in the wearing apparel and bedclothes. (Dr. Decaisne, p. 275.)

Sycosis.—Foment the chin well with warm water, and then apply a strong alkaline lotion, consisting of an ounce of the bicarbonate of potash to four of distilled water, with the view of dissolving the crusts. The hair must be clipped closely to the level of the skin. Then the parts may be brushed night and morning (after ablution) with the following lotion: R. Glycerinæ, f. ℥x.; liquoris potassæ arsenitis, f. ℥ij.; M. ut fiat lotio. When the chin becomes red and irritated with the arsenic, it must be discontinued for a time and the alkaline lotion reapplied. (Prof. Laycock, p. 270.)

Creasote Lotions.—The use of a lotion consisting of 50 grammes of water, 50 grammes of alcohol, and 50 centigrammes of creasote twice or three times a day, is a very successful plan of treatment. The amount of creasote in the lotion may be doubled if necessary. Creasote possesses the power of checking the growth of the vegetable cryptogams. (Dr. Masse, p. 273.)

VENEREAL AFFECTIONS.

Chancre.—Both soft and hard chancre are the result of one and the same virus. Whether a soft or hard chancre is produced depends upon the intensity of the virus. If the virus be very intense, a soft chancre results, the intensity of the virus giving rise to so much inflammation as to prevent absorption. The indurated chancre, on the contrary, is the product of a weaker virus, in which case the inflammation developed is not so great as to lay any impediment in the way of absorption into the system taking place. (Dr. D. J. Simpson, p. 314.)

Communicability of Secondary Syphilis.—The question is doubtful, but cases occasionally occur which point decidedly to syphilisation of the female by the seminal fluid without pregnancy, and without any secretion from the skin of the penis. (Dr. W. Allingham, p. 316.)

A husband may communicate syphilis to his wife without himself having at the time a primary chancre, and without the wife being pregnant. For let the parts become diseased, or let them be subject to increased action in a syphilitic person, and then the secretions from those organs have a power which they had not before, and may communicate the disease directly to any part with which they come in contact. So far

as we know, however, some increased or diseased action in the organ producing that secretion is a *sine quâ non* to its possessing infecting powers. (Mr. Henry Lee, p. 317.)

Condylomata.—Apply with a brush a solution of twenty-five centigrammes of corrosive sublimate in fifty-two grammes of collodion well shaken together. (Dr. G. Finco, p. 326.)

Gonorrhœa.—*Oil of Red Sandal Wood; and Gurgina Balsam.*—From 20 to 40 minims of the oil of yellow Sandal wood, given three times a day, diluted with three parts of rectified spirit, will usually cause the most marked suppression of the discharge within forty-eight hours. It is equal in efficacy and very often superior to copaiba or cubebs, and is pleasant to the taste. It is obtained by distillation from the wood, which grows in the East Indies. Another remedy of considerable value is Gurgina balsam; it is the product of an immense tree growing in various parts of India. Incisions are made and heat by fire is applied to the root. It may be given in doses of a teaspoonful three times a day, uncombined. (Dr. T. B. Henderson, p. 328.)

Infantile Syphilis.—The use of mercurials is quite unnecessary. Chlorate of potash given in two grain doses thrice daily is all the medicinal treatment required. The nose usually requires well syringing with warm water. (Mr. R. W. Dunn, p. 326.)

Inflamed Scrofulous and Syphilitic Glands.—*Iodine.*—Spread laminæ of iodine uniformly on a layer of cotton wadding, and, covering the wadding with a leaf of gelatine larger than itself, apply over the enlarged gland. The circumference of the gelatine should adhere to the skin. The apparatus must be left in its place for from twenty-four to forty-eight hours, and the result will be a phlyctena filled with a thick purulent or bloody serosity. This treatment will generally cause the disappearance of the swelling. (Dr. Prieur, p. 276.)

Syphilis.—*Mercury.*—Mercury is of doubtful efficacy in the primary and tertiary stages of syphilis. The pustular syphilides are the least amenable to it, and the scaly, chronic papular, and certain forms of chronic superficial ulceration the most so. For the tertiary stages, marked by diseases of bone and periosteum, unhealthy suppurating sores on the integument, the iodide of potassium is the best remedy. The long array of charges laid against the use of mercury are highly exaggerated. If mercury be exhibited with ordinary care and discrimination, and particularly if it be applied through the skin, there is little or no risk of injury to the constitution. (*Editor of Medical Times and Gazette*, p. 303.)

Syphilisation.—During the last eleven years the experiments of M. Sperino, of Turin, have verified the announcement of Auzias Turenne, that the syphilitic taint can be removed by syphilisation. Lately Professor Boeck, of Christiania, has come to the same conclusion. Let a patient suffering from severe constitutional symptoms be inoculated, say on the chest, with matter from an indurated chancre, and be re-inoculated every third day with matter from previous pustules, and the successive crops of pustules will gradually become smaller and at last prove abortive. It is better to use only the matter from an indurated chancre, for it is worked out in fewer generations than matter from a soft chancre. Further, by this means the chance of relapse is reduced to a minimum: for, during the last three years, during which he has confined himself to inoculation from indurated chancre, Professor Boeck has not recorded a single relapse, whereas there was nine and a half per cent, of relapses when matter from a soft chancre was employed. The employment of syphilisation as a prophylactic is revolting, but as a curative means its use is perfectly justifiable. The case of a patient suffering to an alarming extent from symptoms of syphilis and threatened phthisis is related. He was completely cured, and gained three stones in weight. (Dr. D. J. Simpson, p. 312.)

Syphilitic Eruptions.—*Their diagnosis.*—The coppery tint of an eruption is frequently fallacious, as this tint is found more or less marked in every case of non-syphilitic psoriasis. Again, syphilitic eruptions do not present it at all at their outset; it is only when they become developed that the coppery tint appears. The absence of itching and smarting is a peculiarity which often aids in the recognition of a syphilide, as does also its tendency to occur in segments of circles, and its preference for the face, occurring frequently at the angles of the mouth, at the inner canthus of the eyelids, in the neighbourhood of the ala of the nose, and on the forehead near the roots of the hairs. The mixed character of syphilitic eruptions is another means of distinguishing them, pustules, vesicles, and scales being frequently mixed up together. No two diseases are more frequently confounded together than syphilitic lupus and the lupus that occurs in the scrofulous. The presence of cicatrices in the throat will often solve this question. Severe cases of eczema in infants are frequently mistaken for infantile syphilis. (Dr. B. Squire, p. 322.)

Syphilitic Lichen attended with Intense Pruritus.—The opinion is pretty general that the absence of itching is one of the most valuable diagnostic signs in syphilis. A case

of undoubted syphilitic lichen is related at p. 325, under the care of Dr. Sisson, at the Royal General Infirmary, in which the pruritus was excessive. It was quite relieved by small doses of bichloride of mercury. (Dr. Sisson, p. 325.)

Vaccino-Syphilitic Inoculation. — The following is well authenticated: Six children were vaccinated from one child. Five out of the six between three and four weeks afterwards had indurated ulcers on the inoculated parts. Each one of these five communicated the disease to members of its own family, so that twenty-three persons were infected from this single source. The mothers and nnrses of these children had first a chancre on the breast. The fathers of the children and the fathers of the nurses remained healthy until some form of secondary disease appeared on the private parts of the mothers and nurses, and then the husbands in some cases contracted the disease, which appeared in the form of a primary chancre. (Mr. H. Lee, Dr. Viennois, p. 318.)

AFFECTIONS OF THE EYE.

Cataract and Artificial Pupil.—*Dressing for the Eye after Operation for.*—After the operations for extraction of cataract and for artificial pupil the best dressing is a little jeweller's cotton wool placed on the closed lids, and covered with a few turns of flannel bandage. Very gentle compression produces great comfort. (Mr. J. V. Solomon, p. 297.)

Extraction of Cataract.—*Mooren's plan.*—In cases where from the state of the patient's health, or the condition of the eyeball itself, the ordinary operation is almost sure to fail, it is better to make a prior iridectomy. The most common causes of failure are great age, complicated with debility, severe cough, asthma, persistent vomiting, or constitutional cachexia. The existence of pupillary adhesions, a diseased or deteriorated condition of the lens or eyeball itself, are also indications for the careful performance of iridectomy. The worse the patient's condition, the longer must the interval between the iridectomy and subsequent extraction be. It is found that this prior iridectomy almost entirely prevents any risk of subsequent inflammation and destruction of the eye. When the cornea is extremely thin, extraction alone is rarely successful; we cannot diagnose this with certainty prior to the completion of the section, but we may infer it when the skin is extremely fine and thin. When the portion of iris removed is from the upper part no deformity or inconvenience results. (Dr. C. Taylor, Dr. A. Mooren, p. 284.)

Jacobson's plan.—The cause of failure after extraction of cataract is inflammation of the eye, leading to suppuration and destruction of the organ. This inflammation always commences in that portion of the iris corresponding to the flap, and compressed by the lens in its exit. This to prevent all risk must be got rid of. In performing the section, Jacobson makes an inferior flap with the line of incision not in the transparent tissue, but in the vascular limbus corneæ, so as to get a larger opening for the lens, and to place the wound nearer than usual to the materials for repair. Nearly all the external wound is made indeed in the conjunctiva. The knife, which must be of a breadth corresponding to the semi-diameter of the cornea, must be withdrawn before making a complete division of the parts, a small central isthmus of conjunctiva alone being left. Von Gräfe's fleam-shaped cystotome is then introduced and the capsule freely opened; and then the isthmus is divided, after the escape of the lens the lower segment of iris is drawn out with proper forceps and cut off close to the ciliary margin. If the cornea be in right position, Arlt's compress is applied immediately; but if it be depressed, five to thirty minutes must be allowed for a re-secretion of aqueous humour. Jacobson is very particular not to operate until the patient is fully under the influence of chloroform, in order that the ocular muscles may be quite passive. (Dr. C. Taylor, M. Jacobson, p. 292.)

Grey Cataract.—Schufte's Method of Scooping out.—With one of Jäger's bent iridectomy knives incise the cornea close to the sclerotic, so as to form a curved slit from three to four lines in extent. With a hook a portion of iris is then drawn out and removed, a slight belt at the ciliary margin being however left to afford support to the vitreous humour. The capsule of the lens must then be freely opened, and any superficial softened matter coaxed out with the scoop of the ordinary curette. Then Critchett's vectis-spoon must be glided between the posterior surface of the cataract and the hyaloid membrane, and the lens cautiously lifted into the anterior chamber and extracted. Any remaining portions must be carefully fished out with the spoon, while the globe is lightly rubbed with the lids from time to time, so as to wash any fragments concealed behind the iris into the pupillary area. This operation enables us to deal with all varieties of cataract, however dense. The chief risks attending it are, that the hyaloid may be ruptured and the vitreous humour allowed to escape. This operation is at present in great repute at Moorfields. (Dr. C. Taylor, M. Schufte, p. 294.)

Extreme Divergent Strabismus.—The indications to be fulfilled by any proceeding undertaken for the radical cure of *extreme* divergent strabismus, when a result of previous tenotomy of the inner rectus, are:—To give the eye a central position; to restore the length and the proper point of attachment to the adductor muscle, thereby insuring balance of the opposed muscular forces and *permitting normal convergence*. Anæsthesia having been induced, and a self-acting speculum applied, make a rather long perpendicular incision down to the sclerotic, and distant about half a line from the inner edge of the cornea. Dissect the conjunctiva from the eye by means of curved scissors, and make two short incisions through the extremities of the first incision towards the nose. A similar flap is now to be made on the outer side of the cornea, and the external rectus divided. The two flaps must now be united over the centre of the cornea by three fine sutures. The eye now occupies the axis of the orbit. The central suture must be removed on the second or third day, so as to obviate corneitis. At the end of three weeks the conjunctiva should be trimmed of any uneven processes. (Mr. J. V. Solomon, p. 301.)

Prolapse of the Iris.—In some cases the steady and continued contraction of the pupil produced by Calabar bean has succeeded in inducing withdrawal of the protruded iris from the wound. The patient is, however, seldom seen early enough to allow of this treatment. The prolapsed iris should not as a rule be removed, for the wound which has produced it being a punctured one, and inflicted usually with considerable violence, does not unite at once, and remains more or less patulous; hence, if the prolapsed iris is snipped off the constant drain of the aqueous will often cause an additional protrusion to follow. As the portion of the iris which protrudes is usually not the edge, but a fold, bulged to its fullest extent by the aqueous behind, it is an excellent plan to prick it occasionally with a fine needle, that it may collapse and permit the edges of the wound to close upon it. In the majority of cases of prolapse from injury, it is the best plan of treatment to close both eyes, and place a little cotton wool or charpie evenly over the injured one, so as to form a compress which is to be kept in its place by a turn of a roller. This often affords considerable comfort, and so long as this is the case it may be worn. (Mr. G. Lawson, p. 282.)

Strumous Ophthalmia with Irritable Vascular Ulcer.—The seton forms a most valuable and almost infallible method of treatment in cases of strumous ophthalmia with irritable vascular ulcer. The seton employed should consist simply of

a thread of ligature silk passed double through the skin of the temple, that part under the hair, in order to avoid a visible scar. (Mr. W. S. Watson, p. 281.)

Trichiasis.—By the following plan of operation this distressing complaint may be completely cured without leaving even a trace of deformity. Insert under the lid a horn spatula, and commencing at the inner angle of the eye dissect along the upper surface of the tarsal cartilage, separating the skin and eyelashes from the parts beneath. Then remove an elliptical piece of skin from the eyelid, leaving the tarsal margin and eyelashes untouched and attached at either end. This tarsal portion must then be approximated to the upper part of the eyelid by four or five fine stitches: union will rapidly take place. The steps of the operation will be found more in detail at p. 279. (Dr. E. P. Wright, p. 278.)

MIDWIFERY, AND THE DISEASES OF WOMEN, ETC.

Child-Crowing.—Let the child be exposed as much as possible in the open air; if it be dry, the colder the better. For many hours daily it must be carried to the most exposed parts of the neighbourhood, of course being sufficiently clothed to prevent its taking cold. This, however, rarely happens. (Mr. J. Roberton, p. 397.)

Disproportion.—*A more Extended use of Version.*—In all head presentations in which the forceps will not succeed, instead of at once employing craniotomy it is better to try turning, even though there is little hope of extracting by means of it alone. In cases which, without turning, would require craniotomy, the application of forceps to the head, *after turning*, will often be sufficient to effect delivery, with a better result to the mother and frequently without injury to the child. And even in cases where version does not enable us to avoid craniotomy, its previous employment seems to render that operation easier of performance, without adding in any degree to the risk. The occiput of the child may be perforated behind the ear with the greatest ease, and the hold afforded by the body renders the delivery of the head comparatively easy. (Dr. A. Inglis, p. 337.)

Dysmenorrhœa.—In cases of painful dysmenorrhœa from contracted os and cervix uteri, it is necessary to enlarge the parts by some operation. Dr. Marion Sims accomplishes this in a somewhat different manner to that pursued by Dr. Simpson. Dr. Sims brings every thing into plain view, and holding the uterus downwards by means of a tenaculum adapted for the purpose, divides the cervix by scissors. He

tampons the vagina after the operation, for the double purpose of guarding against hemorrhage and ensuring an open os. The details, with explanatory woodcuts will be found at p. 387. (Dr. J. Marion Sims, p. 385.)

Early Pregnancy.—*An Additional Symptom of.*—It is in our power occasionally to foretell pregnancy as early as from the sixth week to the ninth. At this period, by auscultation *per vaginam*, a sound may be heard very like the sudden cessation for a moment of a general splenic murmur. It is synchronous with the pulse, and when very distinct, which it sometimes is, it has a very high pitch. It requires the closest attention to detect it, and a very practised ear. The instruments required are two, a glass stethoscope and a wooden vaginoscope. Glass is a beautiful conductor of sound, and does not make any noise when rubbed against the clothes. When made long and with a curve it is very easy of application. (Dr. C. H. F. Routh, p. 331.)

Excoriated Nipples.—Every time the child has left the nipple apply with a camel's-hair brush, gum arabic in an impalpable powder. This shields the nipple from atmospheric influences from the moment it is applied, and protects the nipple from the excoriating effects attending the act of sucking. (Dr. Mac Cormack, p. 361.)

Fibrous Tumours of the Uterus.—Fibrous tumours of the uterus are of such low vitality that the slightest interference with them stops their growth. Simple incision of the os and cervix is frequently sufficient to arrest their growth, and the gouging or breaking up of the tumour is unnecessary. Having placed the patient in the lithotomy position, let an assistant depress the perineum well by means of Bozeman's speculum. Let the operator seize the anterior lip of the os uteri by a pair of vulsellum forceps, and then, by means of the hysterotome, cut the os and cervix laterally, first towards one side and then the other. The cut surfaces are then plugged with oiled lint, and the vagina also thoroughly filled with lint or cotton wool, so as to ensure most perfect pressure and exclusion of atmosphere. When this last stage is well carried out there is never any serious hemorrhage. When it is considered advisable to perform the second step of the operation, viz., gouging the tumour (which should not be done *sooner* than a fortnight after the incision of the os and cervix, the patient is again to be placed in the lithotomy position. The index finger of the left hand must now be inserted within the inner os, and a pair of long-handled, blunt-pointed, curved scissors passed up to the tumour, and gently pressed through the capsule; the blades are then to be so

opened as to break up the tumour. It is not necessary or advisable to break up much. Oiled lint must then be thoroughly applied as before. (Mr. I. Baker Brown, p. 362.)

Fœtal Head.—*To deliver after Perforation.*—Remove the bones of the calvarium as far as possible, leaving the skin to protect the soft parts of the mother from injury. It is found that the base of the skull and face can then be removed with most ease when the face points anteriorly, and the chin is brought well down. This may be done with facility by using a small blunt hook, which may readily and without chance of injury be passed into the orbit. (Dr. J. Braxton Hicks, p. 345.)

Hemorrhage After Labour or Abortion.—Inject a solution of perchloride of iron into the uterus, after clearing out the cavity of placental remains and clots. The solution should be of the strength of one part of a saturated solution of the salt to eight of water. It not only arrests hemorrhage by coagulating the blood in the mouths of the uterine veins, but it is a splendid antiseptic, and hence reduces the liability to puerperal fever. (Dr. R. Barnes, p. 349.)

Hysterical Paralysis.—*Electricity.*—In cases of hysterical paralysis and anæsthesia the continuous galvanic current, such as is produced by a large number of cells of Bunsen's or Daniell's battery, feebly charged, generally proves curative. This current acts on the nervous centres by reflex action from the sentient nerves of the skin. This action is not possessed by the interrupted current. For local ailments invading the peripheral nerves the interrupted current is more beneficial. (Dr. Savage, p. 50.)

Labour Pains.—The pain attendant on labour is more neuralgic in its character than owing to the specific muscular act itself. The pain of labour may be relieved by the application of cold to the abdomen, by means of a freezing mixture, or pounded ice contained in a flat tin box. The pains are if anything increased in intensity, without any corresponding increase in the attendant suffering, the actual pain being sensibly diminished. (Dr. J. M. Granville, p. 341.)

Ovariotomy.—How to deal with the pedicle is the great difficulty. Dr. Tyler Smith transfixes it with a double silk ligature, cuts off the ends of the latter, and allows the whole to drop into the pelvis. Mr. Spencer Wells prefers the use of the clamp when the pedicle is long. Dr. J. Braxton Hicks believes it would be possible, by firmly compressing the pedicle for an inch or so by a serrated clamp or some such apparatus, to condense the tissues so firmly as to completely prevent bleeding, and so do away with the use of either a clamp or suture. (p. 369.)

There have hitherto been three methods of securing the pedicle in cases of ovariotomy: first, by ligature, allowing the ends to hang out; second, by cutting off the ligature short and closing the wound; and, third, by the clamp. Mr. Baker Brown has lately adopted a fourth plan, and with successful results in the case in which it was tried. The pedicle being secured by a clamp, it was thoroughly seared by the actual cautery and allowed to drop into the abdomen. The wound was healed in a week. The cautery should not be quite at a white heat. (Mr. I. Baker Brown, p. 370.)

Pruritus Pudendi.—One or two superficial applications of lunar caustic promptly allay the irritation of pruritus pudendi. (M. Dieudonné, *Glasgow Medical Journal, April* 1865, *p.* 126.)

Sickness of Pregnancy.—In the morning, an hour before rising, let the patient drink about three ounces of very strong, hot decoction of coffee, and during the day let liquid food be taken frequently in small quantities. If this fails, try in conjunction five drops of tincture of iodine in a teaspoonful of cold water every two hours. Should everything fail to give relief, the patient may be sustained by nutritive enemata for a time, that the irritable state of stomach may have time to subside. (Dr. J. P. Cassells, p. 334.)

Transfusion of Blood.—A description of a very perfect apparatus for the transfusion of blood is given at p. 347. (Dr. Graily Hewitt.)

Uterine Polypi.—*A modification of Chassaignac's Ecraseur.*—When the polypus has descended into the vagina, Dr. Braxton Hicks' cord of wire answers very well, but when the polypus is intra-uterine we cannot apply it, nor can the chain of Chassaignac be applied on account of its want of rigidity. This difficulty may be overcome by adding to Chassaignac's instrument a porte-chaine, which may be described as a pair of dilating forceps with spring blades, which render the chain stiff, so that it may be passed straight into the vagina, or into the cavity of the uterus as easily as we would a sound or a sponge probang; after which the chain is expanded by the blades of the forceps or porte-chaine. (Dr. J. Marion Sims, pp. 382–3.)

Vaginal Injections.—*Rasch's Instrument for.*—An efficient instrument for a vaginal douche may be made from a piece of strong but small india-rubber tubing, about six feet long. It is to be used as a syphon, and has, at the end to be immersed in the basin of water or lotion, a heavy leaden end, and at the vaginal extremity a piece of thick elastic bougie. (Dr. Adolph Rasch, p. 395.)

Vesico-Vaginal Fistula.—It is extremely difficult to see and bring within easy reach of the knife the edges of a very small fistula, particularly when situated very high up in the vagina. Dr. Banon, of Dublin, has contrived an instrument to obviate this difficulty. It is a curved steel sound or probe, provided with four elongated points projecting backwards. It may easily be forced into the smallest aperture, and on withdrawing it will grapple the margin of the fistula on every side and enable the operator to divide it with the greatest ease. (Dr. A. P. Banon, p. 395.)

MISCELLANEA.

Alkaloids of Opium.—Of the alkaloids of opium three only possess the property of inducing sleep; these are, morphia, narceine, and codeia. The sleep produced by the subcutaneous injection of codeia differs essentially from that produced by morphia. Profound stupor cannot be produced by codeia, but the effect is more that of being calmed, and soothed to natural sleep. Animals do not awake after the administration of codeia as they do after that of morphia, in a state of terror, and their natural disposition altered. Narceine is the most somniferous substance contained in opium, and it acts more powerfully with equal doses than does codeia, the sensibility to noises which characterises the sleep produced by the latter being absent. A profound calm and absence of insensibility are alone produced by narceine. As a poison, thebaïne, although not a narcotic, is much more dangerous than is morphia. Codeia is also more poisonous than morphia, but infinitely less so than thebaïne. (M. Claude Bernard, *Edinburgh Med. Journal, Nov.* 1864, p. 454.)

Therapeutic Uses of the Alkaloids of Opium.—Narcotine has an action on the nervous system the opposite to that of morphia, being that of an excitant. Administered in doses of five or ten centigrammes in cases where the vital powers are failing, either after surgical operations, in paralytics, or in dying persons, it will cause the strength of the pulse and the animal heat to increase. It should be given in doses of one grain daily. Thebaïne is an excitant of the upper portion of the spinal cord. It has not hitherto been of any use therapeutically, the agitation and sleeplessness which follow its administration preventing any good result. Narceine is a valuable calmative, in ordinary doses of from one to two grains producing calm and comfort, but not sleep. It appears to have an especial action on the lumbar portion of the spinal cord, producing relief in cases of painful paraplegia. (Dr. Ozanane, *Edinburgh Medical Journal, Nov.* 1864, p. 459.)

DIET OF CHILDREN.—The diet of children of all ages should be, a substantial breakfast, with animal food in some shape; a substantial dinner of meat, vegetables, and cereal pudding; and a substantial supper, also consisting in part of animal food. The drink may be milk, tea, cocoa, and possibly beer. This is the diet of health; a diet capable of making a strong body and also a strong mind; and a diet capable of preventing disease. Rice and amylaceous food should have no place in the diet of health, but should be reserved for the sick room. (Mr. E. Wilson, *Med. Times and Gaz., Jan.* 7, 1865, *p.* 25.)

Baron Liebig's Soup for Children.—Cow's milk does not adequately represent the milk of a healthy woman, and when wheaten flour is added, although starch is not unfitting for the nourishment of the child, the change of it into sugar in the stomach during digestion imposes an unnecessary labour on the organism, which will be spared if the starch be beforehand transformed into the soluble forms of sugar and dextrine. This can be effected by adding to the wheaten flour a certain quantity of malt. The following is the best way of preparing this food:—Half an ounce of wheaten flour, and an equal quantity of malt flour, seven grains and a quarter of bicarbonate of potash, and one ounce of water, are to be well mixed; five ounces of cow's milk are then to be added, and the whole put on a gentle fire; when the mixture begins to thicken it is removed from the fire, stirred during five minutes, heated and stirred again till it becomes quite fluid, and finally made to boil. After the separation of the bran by a sieve, it is ready for use. By boiling it for a few minutes it loses all taste of the flour. (*Lancet, Jan.* 7, 1865, p. 17.)

ENDOSCOPE.—The endoscope is an instrument by means of which any viscus or part of the body into which a straight tube can be introduced, may be examined by the eye of the surgeon. The interior of the urethra, bladder, uterus, nasal passages and rectum, may be examined minutely, and changes in their appearances determined with accuracy, and the treatment of many obscure cases rendered certain and successful. It may be obtained of Fannin and Co., Dublin. (Dr. Cruise, p. 254.)

WIRE-ROPE ECRASEUR.—The wire-rope ecraseur is not intended to compete with the chain ecraseur, although it is nearly as strong; but it is constructed to be used in places where the chain cannot be used, at the fundus of the uterus, or on the cervix, *in situ*, &c. The most common fault of makers is to leave the eye of the shaft sharp-edged instead of perfectly smooth, this causes the rope to snap over heavy work. Another fault is to twist the wires too evenly and firmly; this prevents its readily cutting through large firm growths. (Dr. J. Braxton Hicks, p. 366.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

1.—ON TYPHUS AND TYPHOID FEVERS.

By Dr. Wilks, Physician to Guy's Hospital, &c.

[Dr. Wilks gives us 12 cases of fever which occurred in Guy's Hospital as illustrative of his treatment of typhus and typhoid without stimulants. As the treatment of fevers, pneumonia, and other diseases, by large quantities of stimulants, has lately become popular with many members of our profession, we advise them to read with attention the following remarks by Dr. Wilks, the Editor of the Lancet, and Dr. Gairdner, of Glasgow.]

With respect to the treatment of fever (observes Dr. Wilks) I adhere to the old practice of the hospital—that which was laid down by Drs. Bright, Addison, and Barlow, in their lectures, and which they adopted for so many years with success. These teaching of these professors was to the effect that a large number of cases ran their course without any other treatment than careful watching and feeding; that they required no other medicine than a simple saline; but that some needed a stimulant during the progress of the disease, and others required it from the commencement. The question, therefore, with regard to the administration of stimulus, was *when* to give it, and in what quantity. At the present time there are advocates for a universal method in favour of alcohol in all cases of fever, just as there are those who indiscriminately administer ammonia in scarlet fever, and who, when failing to prove its value in all cases, fall back upon the explanation that if alcohol or ammonia be of real service in a bad case of fever or scarlatina, and if these remedies do no harm in the milder forms, it is a good rule to administer them universally. Such a method is not only unscientific, but I believe positively injurious; for in many cases of typhus fever in young people, where the brain has been involved, I have a very strong opinion that the brandy which I have seen given in such cases has been positively hurtful. In my intercourse with medical men I judge that very many are scarcely alive to the fact that typhus fever is very rarely fatal in young persons—the prognosis,

indeed, only becoming serious as years are added to the age of the patient,—and therefore that they are too apt to attribute recovery to their remedies. Young persons always do well if left alone. Of this fact I could now quote a large number of cases in proof, and, on the contrary, the few instances which I have seen end fatally have been those in which a large amount stimulus was given from the commencement of the disease; and, what perhaps is even more to the point, the withdrawal of stimulus in some cases where it was adopted as the method of treatment, has been attended with the most decided advantage.

Although the subject of fever and its treatment may appear exhausted, yet this difference of opinion respecting the use of alcohol shows that this therapeutic struggle must continue for some time longer; for whilst we are witnessing the free use of stimulants in fever, we are witnessing that it was Dr. Graves's plan to administer antimony in the same disease. When deliberating on the merits of such contradictory treatment, many escape the dilemma by believing that different diseases have been the subject of treatment, and that fever has changed its type; they assume, without even a sceptical thought passing through their minds, that the favourable issue was due to the remedy, and therefore their conclusion is a logical one. Such persons, however, have no right to frame this or any other opinion as to treatment unless they are conversant with the natural history of the disease; for, did they quite realize to themselves the fact that in young persons typhus fever is rarely fatal, they might with great advantage state the argument in another form: as, for instance, Will a few doses of antimony, or a few daily ounces of wine, or the abstraction of a few ounces of blood, be sufficient to kill a patient suffering from a disease the tendency of which is to subside spontaneously in the course of a few days? Surely, too, every medical man must have seen cases, either under his own charge or his neighbour's, where, from a mistaken diagnosis of the nature of the case, a remedy has been given which above all others he would have discountenanced had his opinion been otherwise: he must, for instance, have seen several grains of opium administered daily on the supposition that the case was one of delirium tremens instead of fever, and yet the patient has done well. If he saw many such instances, he might consider that he had good reason to believe in another change of type. Without, therefore, denying that fever may annually change in character as to some minor features, yet I believe that the argument of its great alteration founded upon the administration of remedies to be a most fallacious one. It is for this reason that I offer a few cases out of a large number which have recovered without the use of stimulants, also cases where patients were cupped

and bled and yet had a favourable issue. Instances where a good recovery has taken place in patients who have lost much blood by the nose or bowels must be familiar to every practitioner.

I would not wish to dictate rules of treatment to any of my medical brethren; but my own opinion is that expressed in the first few sentences of these observations. I believe that support and a moderate amount of wine is the best treatment; but I assert that alcohol is not an antagonist to the fever, which runs its course in spite of the administration of the stimulant. I would not say that in many cases of typhus in young people a little wine may not be useful; but the fact still remains, which cannot be gainsaid, that such cases would do well without any stimulant whatever. When the ground is thus cleared we shall know better what we are doing. I might mention that the only two cases which I have seen fatal of late have been those of two students, to whom a large amount of stimulant was given, and who had the care of the most assiduous nurses both night and day. In one of these there were constant convulsive movements during the last five days of life, with coma vigil, and other symptoms resembling those in which the late Dr. Graves would have administered antimony. In this case there was no albumen in the urine, nor in another, which recovered, where convulsions were most violent. Albumen has, however, been very frequently present in other instances.

I will offer one or two instances as examples of several where the patients have made a good recovery without the use of stimulants. These have been mostly cases of typhus, this being the form of fever principle prevailing; but the same remarks are applicable to typhoid. With regard to the use of stimulants, it is important to know how much is needed even if it be agreed that the extreme effect is required. It is said by some that in such a case too much cannot be supplied. But of this I have great doubt; for should it be conceded that alcohol is antidotal to the fever poison, or that it is directly supporting the patient, it may be still possible that a surplus amount may be harmful. I am under the impression that a patient who requires the alcohol treatment may be kept under the influence of the stimulant by taking a tablespoonful of brandy every hour, which amounts to twelve ounces in the twenty-four hours. I have grave doubts about more than this being of service, although I have of course witnessed recovery where larger quantities have been taken.

The other cases of typhoid I give merely to show that loss of blood is not necessarily fatal, nor is sufficient to make us regard those patients in whom it occurs as having a different type of fever.

[The following commentary on these cases, and on the general practice of using so much alcohol in fevers, by the Editor of the 'Lancet' is so good, that we reprint it entire, and also the experience of Dr. Gairdner, as detailed in our next article. We think the profession will have enough to guide it on this matter, and will hesitate to continue the practice of excessive stimulation which has lately appeared to gain ground.]

In the 'Lancet' of January 28th last, our readers will have read and been interested in reading a series of cases of typhus fever with remarks thereupon, by Dr. Wilks, of Guy's Hospital. The remarkable feature in the treatment of these cases was a negative one—the absence of stimulants. We take credit to ourselves for having brought before the profession from time to time the therapeutical fashion of stimulation which has been so prevalent of late years, and for having suggested doubts as to whether it was a fashion based on grounds which would bear scrutiny.

There are few things more remarkable in the recent history of medicine than the extent to which alcohol has been introduced, and the importance which has been attached to it in the treatment of disease. Coincidently with a greatly reduced use of it in society has occurred a greatly increased use of it in medicine. Judging from the London practice of the last few years, a cursory and casual observer might think that alcohol was a remedy of specific power, the triumphant introduction of which was to make Dr. Todd as famous as vaccination made Jenner, or chloroform Simpson. This use of alcohol has coincided pretty closely with the prevalence of a theory of disease which is enticing from its very simplicity. All disease, according to this theory, is a form of debility. A semi-solid exudation in the tissue of the lung, a layer of lymph on the interior of the pericardium, quick pulses and slow ones, are so many different forms of this universal disease of weakness. All semblance of strength in disease is a mere deception; a burning skin, a bounding pulse, are only weakness in a disguised form. Of course weakness is a somewhat vague thing, defying the definition of the physiologist and the analysis of the chemist; but it is a popular term which every man is unthinkingly apt to think that he understands. This is a great recommendation of a theory —that it be short and comprehensive, and have an easy look; and therefore it has had an extensive reception accorded to it at the hands of busy men who have not much time to spend over the choice of their theories. The only thing more remarkable than the extensive reception of this one notion of disease is the extensive spread of a belief that alcohol in some form or other is the panacea for it. We are not questioning just now the

soundness of this discovery. We are simply pointing out that alcohol, as a matter of fact, has become, or let us say had become, under this double theory more important in the weaponry of medicine than almost any other remedy, though there is not a tittle of evidence that it can "antagonize" any one disease in the sense in which quinine antagonizes ague, or mercury syphilis, or (may we say?) alkalies rheumatic fever. We have always had misgivings as to the soundness of this theory. One great discovery has been made of late years—that there is a method in diseased processes; that there is a good "nisus" in the worst of them; that when disease overtakes the body, it finds the body not unprepared to meet it, and make the best of it, but with many provisions for conducting it to a kindly and harmless issue; and that the height of medical wisdom is to recognise these provisions, to place the body in the best natural circumstances, and gently assist all the natural processes of cure. Old medicine erred in treating disease and nature too roughly. This new treatment by alcohol, pushed as it was a few years ago, was scarcely less "perturbing" than the old one. Thirty-six ounces of brandy in twenty-four hours to a girl fourteen years old, with fever and a diseased heart, was apt to be a disease of itself, and was surely calculated to give more work than help to the heart and the excreting organs, to say nothing of the brain, on the efficiency of which we know recovery from disease so much depends. Accordingly we are not surprised to learn of the failure of this plan. It is excusable only as a wild and random rebound from an opposite error, and we should despair of medical philosophy altogether if we did not receive intimation from time to time that the days of this fashion are numbered, and that a more moderate, a more physiological method of treating disease is destined to replace this.

Dr. Gairdner did good service, in the course of last year, in publishing in the 'Lancet' a paper on the treatment of typhus on a plan according to which the majority of the patients (as many as nine-tenths) got no stimulants at all. We are not unmindful that exception has very recently been taken to this paper by another respected contributor to this journal, and this in our own columns. Dr. Henry Kennedy, of Dublin, after waiting nine months, and finding that no physician challenged the views advanced by Dr. Gairdner, has done so in an able, if not also a somewhat warm, communication, which will be found in the 'Lancet' of Jan. 7th. We apprehend that the simple explanation of Dr. Gairdner's views going so long unchallenged is, that they commended themselves to the profession as being neither unreasonable in themselves nor unsupported by the facts and figures adduced by Dr. Gairdner. We cannot entirely agree with Dr. Kennedy in his sweeping

objection to the application of statistics to medical questions. There is room for the application of statistics in medicine. The efficacy of ventilation in hospitals, of lemon-juice in scurvy, of vaccination in preventing small-pox, are great facts which admit of statistical statement, and the statistical statement of them is widening every year the extent of human belief in them, and the application of them. And we fancy that, if the introduction of large quantities of alcohol into the treatment of disease were the boon which the late Dr. Todd thought it to be, the fact would admit at this time of far less statistical disproof than is accumulating against it. There is really in matters of detail very little serious difference between Dr. Kennedy and Dr. Gairdner. They both say that there are many cases of fever which require no alcohol; whereas Dr. Todd's administration of it was well-nigh universal. They both say that death from fever in young persons is exceedingly rare: Dr. Kennedy is not sure that he ever saw one. And yet Dr. Todd found reason to give twenty or thirty ounces of brandy a day to young patients. Dr. Kennedy objects to Dr. Gairdner dividing the ounces of stimulant by the number of the patients; but Dr. Gairdner pointed out distinctly that this was no indication of the amount actually given to each patient, for, whereas most patients got actually no stimulant, these who got any generally got considerable quantities. Dr. Kennedy objects, almost with feeling, to any disapproving allusion to Dr. Todd's practice on the principle *de mortuis nil nisi bonum;* but surely it is a stretch of this rule to ask us to concede infallibility to the dead, and respecting the dead at the expense of the living, to allow any such principle to interfere with the settlement of an urgent medical question. Dr. Kennedy objects to Dr. Gairdner's figures as conveying no detailed information; and so far reasonably enough. The figures are valuable as the calculations of an observer known to be reliable, and who repeatedly says that he bases his opinions not only upon figures, but details which it is impossible to represent in figures. Dr. Kennedy objects, lastly, to Dr. Gairdner's comparing the results of his own practice with that of others in other places, asking, "Is Dr. Gairdner sure of the identity of the disease? Is he certain that disease is the same in London as in Edinburgh or in Glasgow?" Well, Glasgow has no great sanitary reputation; so that, *à priori*, any difference between it and other places might, without violence, have been presumed to be against it. But we are in possession of evidence of a still more satisfactory per-centage of recoveries in cases of fever than that of Dr. Gairdner, and this happens to be in the case of a London hospital and a London physician. Dr. Chambers has published various lectures on fever, and gives a table of 121 cases, of which only 4, or $2\frac{1}{2}$ per

cent., died. And the administration of large quantities of stimulants is not a feature of Dr. Chambers's practice. He agrees with Dr. Gairdner in the essential point of supporting the patient, not by stimulants, but by nourishing food. Other leading physicians in London are acting upon the same principles. Dr. Gull was lately reported to be treating many cases of fever without alcohol; and Dr. Wilks's remarks and cases above referred to well deserve the attention of the profession.—*Lancet, Jan.* 28, *and Feb.* 11, 1865, *pp.* 91, 153,

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2.—TWO MONTHS OF FEVER DUTY IN THE GLASGOW ROYAL INFIRMARY.

By Dr. W. T. Gairdner, Professor of the Practice of Physic in the University of Glasgow.

[The cases admitted into the male wards during the period of Dr. Gairdner's attendance were 68 in number: of these, however, 61 only were typhus or typhoid fevers.]

The treatment, where not otherwise specified, has been conducted mainly on the following principles:—To afford a bland and nutritious aliment throughout the fever, chiefly consisting of milk and water, buttermilk, rice and milk, arrow-root, and beef tea, sometimes with a little bread, where it was relished. The choice of the special form of diet was to some extent determined by the feelings of the patient, and to a like extent left to the discretion of the nurses, but milk diet in all instances formed the staple nourishment. Farther, it has been considered expedient to employ no medicine *as matter of routine*, so that where no statement to the contrary is made, it may be presumed that no medicine at all, unless it may have been an ordinary laxative, or a little common cough mixture or the like, was administered throughout the disease. A similar remark applies to the administration of alcoholic stimulants, except that I have thought it right, in view of certain questions recently under discussion, to state distinctly, as from the hospital wine and spirit roll, which is kept with very great accuracy, the whole amount of stimulants given in each case; and where none were given, to make an express statement to that effect.

[Dr. Gairdner then describes a long series of the most interesting cases, and concludes a long paper by the following hints on treatment.]

The first lesson that has to be learned in dealing with fever, is at the same time to many minds the most difficult—to let well alone. And those who naturally or by education incline to a more or less active routine practice, are not, I believe, likely to

be convinced by statistics, or by any form of abstract reasoning, that such practice is either unnecessary or injurious in such a disease as typhus fever. The argument, to many an irresistible one, will always recur—What! are we to stand by and to do nothing at all, when the pulse is 120, the tongue dry, the brain oppressed and delirious? and it may even be admitted that the question is an extremely embarrassing one for all who are not prepared to answer it out of a matured and carefully studied experience. But in view of facts which have become clearer and clearer in proportion as our knowledge of typhus fever has been defined and rendered more exact, it has become a duty with teachers of medicine, not indeed to acquiesce in a blind expectancy, any more than in any other unreasoning routine, but to take a firm hold of principles based upon experience, and to place these in the light of such practical illustrations as their opportunities, derived from hospital practice, enable them to command.

I would therefore invite the attention of all physicians who admit any doubt as to the proper practice in typhus fever, and especially of all who have still to attain practical experience on a large scale, to the cases here recorded, with the view of showing:—

1. That at least two-thirds, more probably three-fourths, perhaps even a greater proportion, of the cases of typhus fever in the present epidemic, may be successfully treated without either medicine or stimulants; or, at least, with only such slight accidental doses of either as cannot be supposed to influence considerably the result of the fever, as such.

2. That in the cases of typhus fever recorded in this paper the treatment, whether by stimulants, or medicines, or both, has been directed entirely to emergencies and casualties, and not to the subduing of the febrile excitement, or the restraining of the typhoid delirium, or the reduction of the frequency, or increasing the strength, of the pulse; on the principle that fever, with rapid weak pulse and a certain amount of typhoid delirium, are to be regarded as the normal facts of the disease, always observed within certain limits of time, and not to be avoided, or cut short, by any known specific treatment; from which it follows, that remedies, especially very active remedies, are to be reserved chiefly for manifest variations from this normal course; as when tartar emetic has been given, in small doses with wine, in numerous cases of bronchitic complication, or wine and whisky in obvious tendency to collapse; or to take a less extreme instance, a purgative in accidental constipation, an emetic in gastric irritability, lime-water in diarrhœa, or an opiate in case of protracted sleeplessness in the early stage.

3. That under a practice so regulated, the mortality of typhus

fever has been not more than 9·3 per cent. in the cases here presented, which have been treated absolutely without selection, and without excluding a single fatal case on any ground whatever.

4. That in children and young persons, probably up to the age of twenty, or at least fifteen, typhus fever, so treated, is a disease of hardly any danger.

5. That in all cases where a regular daily note of the pulse shows a gradual climax, ending in a maximum rate on the 8th, 9th, 10th, 11th, or 12th days, or even later, and afterwards a gradual decline in the rate, there is no occasion for alarm after the crisis of the pulse has been reached, even although delirium should go on, hardly abated, for several days; and that no sudden crisis is to be expected, or wished for, in such circumstances.

6. That nourishment, of the kinds indicated at the beginning of this paper, is to be carefully administered throughout the course of typhus fever. That free ventilation is also indispensable.

7. That good nursing, and constant watchfulness against casualties; especially watching of the chest, the evacuations, and the skin of the back, are the real "physic" needed by all fever patients; and more particularly by the old, the intemperate, and the subjects of chronic disease, in whom the danger is incomparably greater than in the young, healthy, and temperate.

[The following remarks, from a lecture of Dr. Gairdner on the same subject, are too good for us to omit.]

Milk or buttermilk is with me the staple food in typhus; and I will even say that I know no other food that can be depended on. Yet I see, and always see with a new surprise, descriptions of the treatment and dietetics of fever in which not a word is said about milk, and a great deal about beef-tea, wine, whisky, brandy, and all manner of things supposed to be more strengthening, or stimulating, than milk diet. Now, I tell you frankly that treating fever patients without plenty of milk is a thing that I do not understand at all; for I suppose I have not treated a single case of fever of any kind for the last fifteen years (I cannot make precise statements beyond that date) without milk, and I always proceed on the understanding that milk in fever is the one thing needful as diet—always to be given, and given liberally, whether specially ordered or not. To give wine, whisky, and beef-tea, while withholding milk, is simply, in my opinion, to destroy your patient; and the more wine or whisky you give, while withholding milk, the more sure you will be to destroy your patient soon, because you are thereby superseding the natural appetite (or what remains of it) for a

nourishing and wholesome diet, by a diet—if it can be so called —which so poisons the blood and checks the secretions, and alters for the worse the whole tone of the nervous system and of the digestion and assimilation. I believe that infinite mischief has been done in typhus fever, and in all fevers, by giving wine, and withholding or not giving milk. Under a false theory of administering alcoholic food, it has resulted, not only that natural and genuine food has been withheld, but that the small remaining amount of appetite for such food has been obliterated, and not unfrequently, even at an early stage of the disease, the patient has been practically disabled from taking any proper nourishment at all. I know, unhappily, as a fact, that not only doctors, but nurses and patients and patients' friends are readily brought under the influence of this fatal delusion, that alcoholic liquors can, in fevers, take the place of natural food. But it is none the less a fatal delusion, and I warn you solemnly against it; all the more that the patient is absolutely in your hands, and you can obtain no guidance from his natural instincts, if you begin by overwhelming them with large doses of alcoholic stimulants. I have been very careful, at least for fifteen years past, to avoid this error, and I believe that any success I may have had in managing fever has been more due to this than to any other cause. But I allude to the matter now simply to say that if you would observe the natural or normal course of typhus fever, with respect to the crisis (as proposed at present), you must absolutely make up your mind to feed your patient naturally, and not to stimulate him. An opposite course leads to consequences which I may consider more in detail on another occasion; but what I am anxious to say now is, that it tends to disturb the course of the fever, and to retard the crisis.

Further, you must, if you would observe the natural course of typhus fever, give your patient plenty of fresh air. This is now so very general an admission that I need hardly have alluded to it, were it not that adequate ventilation is still apt to be forgotten in treating fevers in the houses of the poor or in badly constructed hospitals. With adequate ventilation, you must of course, especially in winter, have the means of warmth; and great attention must be paid to cleanliness, so far as this is possible in bedridden and exhausted patients.

These, I think, are all the precautions necessary, I do not say for the proper treatment of typhus fever, but for the proper observation of its natural course in normal cases.

Now I venture to assert—and I make this assertion not by way of theory or opinion, much less prediction, but as a statement of facts, founded on a rather extensive experience ever since 1855 or 1857—I venture to put it forward as a law, or

generalization, with respect to typhus fever: *That in a large proportion of cases typhus fever, left to its natural course, and treated with abundant milk diet and without drugs or stimulants, will have its natural crisis before the twelfth day.* Of course you will understand that I use the word *crisis* in a modified sense; for I have just been explaining that there very rarely is in typhus a true crisis, in the ancient sense of the word. What I mean is, that most commonly before the twelfth day, very frequently as early as the ninth or tenth day, sometimes, though rather rarely, even before the ninth day, indications are to be perceived that the disease is about to terminate favourably; and that these indications are to be trusted, even when some of the most formidable-looking symptoms continue present to a later period. And this I confidently assert to be a very general law, or *norm* (to use a word borrowed from the Latin, but yet not naturalized in English, except in the adjective form *normal*), of typhus fever as observed in Edinburgh and Glasgow of late years. Observe, I do not venture to assert that it has always been so, or will always be so; on the contrary, I have myself remarked the tendency to a delayed crisis as an epidemic fact at certain seasons; but in a large proportion of cases, and excluding certain exceptional groups of cases, it has been so, in the main, in every year from 1857 to 1862 in Edinburgh, and during a large portion of the epidemic of the last two years in Glasgow.

If you wish to have the satisfaction of getting the earliest information of this early crisis in typhus, *watch carefully and constantly, from day to day, or several times a day if need be, the rate of the pulse.* For this is, speaking practically, the guiding clinical fact in the natural history of typhus fever. Making allowance for certain obvious, and a few not so obvious, causes of accidental disturbance, you will find that the pulse pursues a very regular course of rising frequency up to the acmé, and declining frequency beyond the acmé, of the fever; and that although the other symptoms very surely follow the pulse in uncomplicated and regular cases, yet they often follow at some distance behind: so that the crisis can be ascertained for practical purposes, and particularly for the great purpose of guiding treatment, more accurately and effectually by frequent noting of the pulse-rate than by any other method. I have ample proof of this in the cases of the last two months, for which I will refer you to the hospital case-books, and to the abstract in the 'Glasgow Medical Journal' for January, 1865.

This accurate and frequent noting of the pulse-rate in typhus, in connexion with the day of evolution of the disease, is of immense importance as regards the prognosis; for not only are

you able to predict the recovery with greater confidence, and at an earlier period, than you could do otherwise. but you have often the first intimation of complications, either during the fever or during the convalescence, through some unaccountable departure from the *norm*, or regular gradual increment and decrement, of the pulse. In fatal cases, where the death is from *pure* typhus, the pulse usually becomes more and more rapid, and at the same time weaker and smaller, up to the very hour of death. In complicated cases, where the complication is not very dangerous, the pulse may be merely quickened for a day or two, or the crisis may be protracted, or the pulse may begin to rise again, after a quite distinct critical subsidence. In very dangerous complications the pulse is out of all bounds, and no rule can be assigned for its irregularities. But in regular and normal cases, being uncomplicated—i. e., not having any positive organic or inflammatory complication,—the decline of the pulse, even by a few beats, before, at, or about the twelfth day, carries as an after consequence the decline of the fever; and, on the other hand, a delay of the crisis very much beyond this is to be regarded with suspicion, though not by any means necessarily of fatal tendency.

But the most curious inference which I have been able to draw from the cases of typhus which we have been observing for the last two months is this:—If, in a particular case, you observe that the pulse keeps steadily to the *norm* or law of uncomplicated typhus, as I have endeavoured to exhibit it; if the rate of the pulse increases steadily and gradually up to the ninth, tenth, eleventh, or twelfth day, and then, at any period between the eleventh and fourteenth day, is observed to be undergoing a correspondingly regular decrease, not rapid, but regular and gradual; *in such a case of regular crisis indicated by the pulse-rate, it is almost insignificant, as regards the prognosis, that the other symptoms continue formidable-looking for days;* for in all such cases that I have observed, without exception, the patient has got well in the end, and that without further treatment. Thus I can tell you from experience, that, after a regular crisis of this kind, the tongue may be as black as pitch, and so dry that it feels like a bit of roasted leather, delirium may go on, the patient may be out of his bed many times in a night, the skin may be hot and dry, the eruption brilliant or livid for days together; still, if you have the favourable state of the pulse, and if there is no complication, the *patient will get well.* The pulse is your criterion. Whenever it is fairly turned, and declining in rate, especially if it is at the same time gaining in strength and volume, you may be at your ease, unless there is reason to suspect a complication; and even then you may be at your ease, if the complication is not

severe. But observe, *only if you do not interfere*. The proper course in such a case is to take confidence from the pulse, and avoid interference. *You can afford to wait*. And only one more word to-day, but it is a very startling one, and yet true according to my experience: even if the delirium should become worse after the crisis, as indicated by the pulse; nay, more, even if the patient has had little or no delirium before, and *begins* to be delirious for the first time after the pulse has begun to show symptoms of crisis; still, it is better to wait, for in all probability he will be quite well in a few days, provided you do not disturb the course of the fever.—*Glasgow Medical Journal, Jan.* 1865, *p.* 385, *and Lancet, Jan.* 21, 1865, *p.* 58.

3.—ON TYPHOID FEVER IN PENICUIK.

By Dr. George Thin.

[Dr. Thin's observation of fever in Penicuik extends over a a period of six years, and with the single exception of last summer he has always seen more or less of the fever every year. The village of Penicuik lies ten miles south of Edinburgh at the foot of the Pentland Hills, in the valley of North Esk, at a very considerable height above the sea. The last epidemic which occurred in the village was in the months of July, August, and September, 1863. The first case that occurred was that of a boy aged nine years, who lived in a cottage situated in the outskirts of Valleyfield, in a family of characteristically clean and orderly habits. He took ill in the beginning of April, and died in June. No member of the family had been away from home, nor had any visiter been living in the house; in short, no trace of possible infection could be discovered. The cottage is provided with neither privy nor water closet. From this case the fever spread to the family where his linen was washed, and then to many other houses. Upwards of sixty cases came under Dr. Thin's treatment during the three months named.]

I have two things to account for: first, why Penicuik should be notorious for fever at all; and, secondly, the varying conditions that regulate its local distribution. I shall consider these two questions in their order.

I have previously stated that I consider the natural climate of Penicuik to be, with a trifling exception, salubrious. The inhabitants are well and constantly employed, and are consequently comfortably housed, fed, and clad. In general comfort, indeed, they contrast favourably with those of any other Scottish village with which I am acquainted. Such being the case, we are compelled to look for the predisposing causes of disease in the adventitious circumstances specially incident to the conditions of life affecting them.

Avoiding any statement open to controversy, I take it for granted that no intelligent physician will dispute, that one of the ascertained causes concurring in the production of many cases of typhoid fever is exposure to the effluvia of decomposing fæces. Any one who has read the mass of evidence collected on this point by Dr. Murchison and Dr. Budd would probably grant more than this, but so much as this even Dr. Christison grants. In the address delivered by him at the meeting of the Social Science Association in 1863, after exhausting the arguments that can be urged against the connexion between sewer gases and fever, he says, "foul air merely favours its invasion." Why, that is as much as any one ever can say of a supposed cause of an unknown impalpable blood-poison. We all know that other conditions than that of "foul air" are also necessary, but whoever will take the trouble to examine the evidence of facts will find that to be one of the most constant.

In nearly every village of Scotland there is scarcely a cottage which has not attached to it a potato garden, in some corner of which (beside the pigstye, if there is one) is the dunghill or "midden." All the filth of the family is here accumulated for the benefit of the next year's crop, and generally becoming incorporated with earth and ashes, is thus rendered harmless to health. This primitive and natural state of things has long ceased to be the rule in Penicuik. The success of a large paper manufactory has so conduced to the prosperity of the village, that land became too valuable to admit of the old cottage system; large houses of two and three stories, with several families in each flat, having been built to accommodate the increased population. Only one public privy exists in the village; and I speak to what is strictly within my own knowledge when I say, that the private privies have been too few in number, and generally kept in a filthy condition. A pail, with the accumulated excrements of the females and children during twenty-four hours, is an utensil in daily use in very many families.

I believe this state of matters to have been the determining cause of many of the epidemics of fever in Penicuik.

That the point at which this fever breaks out is not determined by the permanent qualities of any particular soil or situation is well illustrated by the topographical description I have given above. During the previous years of my experience, Valleyfield had been singularly free, while the street *b*, which in 1863 had only a few solitary cases on one side, in former years deservedly bore the reputation of being the chief centre of fever. The first, and for six weeks the only case, occurred, as I have previously stated, on the outskirts of Valleyfield. Why the disease should have originated in that particular cottage I am

unable to say. It was situated, as regards cleanliness, &c., neither better nor worse than those adjacent. It can be only attributable to a high degree of that susceptibility in the patient which must exist in every case before the poison can take effect.

The second I consider to be a clear case of infection by fomites. All the fæcal discharge from the first patient was emptied in a wood opposite the cottage, and quite out of the way of the second family. Between the first patient's house and that in which his clothes were washed there are two rows of cottages, in none of which there was as yet any fever, so that it is not difficult to believe that the disease was thus conveyed.

The next cases that occurred were numerous and simultaneous in their appearance, and can, I think, be satisfactorily accounted for.

No amount of fæcal accumulation will produce fever without the co-operation of certain conditions of the atmosphere. What these conditions may be I do not pretend to say, but heat and stagnation are certainly parts of them. There is no reason, for instance, to believe that the sanitary arrangements of Penicuik are any better this year than they were last year, and yet up to the time I left in October, there had not been one case of fever in the village. This is easily attributed to the weather, which, although dry there as elsewhere, was never, with the exception of three days in May, close or sultry; strikingly contrasting, in this respect, with the stagnant heat that preceded the outbreak in July 1863.

It was immediately after the heat that the cases occurred. The cottages were badly accommodated as to privies, and I ascertained, by personal inquiry, that the excreta of the adult women and of young children were thrown at night into the bed of the Esk. During the months of June and July, the volume of water in the river was so small that it was all carried away by the mill-lade at a point higher up, the bed of the river presenting simply a succession of shallow, stagnant pools. The fæcal matter not being carried away by the stream, became decomposed by the heat of the sun, and the resulting effluvia must have been inhaled more or less by all the residents of the cottages.

The square block of buildings which I have described as being about sixty yards farther down the stream, and in which three out of the four families inhabiting it suffered severely, is provided with water-closets. These discharge by a single pipe into the bed of the river immediately in front of the dwellings. At this point also the river was stagnant.

A very similar case to the above is recorded by Dr. Ogier Ward, as having occurred at Birmingham in 1838. A small

stream, used by those living in its neighbourhood as a common sewer, had dried up. About fifty persons took fever in consequence.

I believe that typhoid fever is contagious, but its course at the time of which I am speaking was due to causes of quite a different nature. There was nothing in the way of personal communication which could in any way explain its being localized in special centres. The cottages in Valleyfield had daily communication with the shops in Penicuik, and a great number of the workers in the Valleyfield paper-mills live in Penicuik, and pass and repass several times daily, but this intercourse did not affect the distribution of the disease. It is worthy of remark, that though the mills are in the immediate vicinity of the infected houses, the workers who lived in other than the localities indicated, as a rule escaped.

The source of the poison that produced the cases in the street marked *a* was not far to seek. Immediately behind the last of the two-storied houses, several horses, cows, and pigs are kept in a very limited space; and connected with these and some large families of human beings inhabiting the house, there was a constant accumulation of filth. The liquid part of this, after being conveyed in a close drain for a few yards, flows in a small open ditch through a piece of sloping boggy ground, in order to be conveyed to the river. The smell from this drain was abominable. During the summer I heard many complaints on that score from those living near it. It is situated immediately below the fronts of the two concealed cottages; the workshop and the two-storied houses open to it from behind, and all the inhabitants of these houses were necessarily exposed to its effluvia. It is necessary to add, that there was no possibility of the cases in *a* being produced by the sewage from the cases in Valleyfield, as there is no link by which to trace any connexion between the two centres of disease. The cases in *a* occurred at the extremity farthest removed from Valleyfield. Besides, as I have before remarked, the cases began simultaneously in both.

I have said that six cases occurred, in six different houses, in the street spoken of as *b*. In three of the houses I could discover nothing special in the drainage. It was different with the other three. Two of them were contiguous, and between them there was no offensive drain. This was immediately rectified, and no more cases followed at that point.

The remaining case is interesting. I had been in long attendance on the mother of the family for an ailment of a chronic nature. Every time I entered the house, I perceived a very offensive odour proceeding from the window sink, the drain from which was very short, and had no communication with the drainage of any other house, and I repeatedly prophesied, that

if it was not rectified it would produce fever. The odour was not fæcal, and no fæces had ever been thrown down. Shortly afterwards one of the children took fever.

The offending sink remained in use, and within two months the only two children in the house took diphtheria,—the first cases of that disease in Penicuik for at least twenty years.

I think the facts I have just briefly related afford useful evidence to any one anxious to form an opinion on the disputed points connected with this fever. These points, as far as I know, relate chiefly to its origin and contagious properties. The controversy in regard to the first of these has, I think, been chiefly due to different observers attaching too much importance each to the facts of the cases specially observed by himself, and on the strength of these proceeding to too hasty a generalization. Whether or not there be a ponderable but impalpable essence which must in every case of typhoid fever be present as the proximate cause, I think we can never know; but even granting the existence of such an agent, there is no evidence to show that the secondary causes co-operating to its origin are uniform.

That certain effects of atmospheric agency or decomposing sewage have, in many instances, been the chief factors in the production of this fever has been proved, over and over again, on circumstantial evidence as conclusive as any ever brought before a jury. But we all know that identical conditions of filth and weather existed in Scotland for years without a case of this fever ever being heard of. Clearly, then, there must be present an unknown something, a "change of the type of disease" if you like, though the phrase is apt to mislead. It is very important to bear in mind this *tertium quid*, and bring it prominently forward, if we wish to carry conviction in pressing sanitary reforms. Dr. Murchison, Dr. Budd, and other writers, have done an immense service in showing that fæcal accumulations can in certain conditions produce fever; but by exalting the relative importance due to these theoretically, inquirers of a speculative turn of mind have rebounded into the mischievous extreme of denying that they have any specific action at all. Dr. Christison, in the address alluded to above, says, "I suspect, then, it must be allowed of this disease, as in respect to most other epidemic diseases, that we do not yet know its cause —that foul air merely favours its invasion; but that its true cause is something more specific—some ζύμη or ferment which has hitherto eluded our search. In that case, while we shall do well to encourage better drains, more and better water-closets, and better ventilation of dwellings, still we must not count upon thus extirpating enteric fever."

The unimportant part which Dr. Christison here assigns to defective drainage is in direct variance with facts open to exami-

nation by any one who chooses to take the trouble. It might be said, with almost equal force, that the true cause of ague is some ζύμη or ferment, that fens and marshes merely favour its invasion, and that, drain and cultivate them as we may, we must not count upon thus extirpating it.

In classifying typhoid fever with other epidemics, Dr. Christison is guilty of a very palpable fallacy. There are no epidemic diseases common in this country which have the same evidence to connect them with special local conditions. It does not at all affect the question to point to individual instances of the disease occurring in houses whose ventilation and drainage are perfect. These instances, exceptional even in number, do not in any way invalidate conclusions drawn from a large number of facts, and may easily be explained on the ground of unusual susceptibility on the part of the patient, who may have been exposed to some other source of the poison without his knowledge. To found any argument on a few solitary cases like those adduced, would be as reasonable as it would be to deny the connexion between tetanus and wounds, because a few cases of tetanus had occurred in which no wound could be observed.

Dr. W. Budd of Bristol has observed and recorded a very interesting series of cases, in which he supposes a number of individuals to have been infected by the alvine discharges from a patient suffering from fever. Reasoning from these, he has advocated the opinion that sewage can produce fever only when it contains discharges from those previously diseased.

In the Lancet, Nov. 9, he assigns two reasons for this belief, both of them very inconclusive. First, he gives instances in which cloacæ, after having existed harmlessly for years, have suddenly become sources of disease, concluding therefrom that they must have received some new noxious element. Dr. Budd's views are so materialistic that he forgets to assign any part to the *tertium quid*, of which I have spoken as always co-operating to produce fever. Dr. Christison can see nothing else than his ζύμη, but Dr. Budd cannot see it at all. This is the real new element which has rendered so many cloacæ fruitful. His second reason is stated thus: "That this fever is caused by something far more specific than the putrescent compounds which exhale from common sewage, seems to be further plain from the significant, not to say decisive fact, that of the two kinds of fever, the intestinal is the only one that originates in sewer influence."

The only fact that that shows is simply what is stated, that typhoid fever has a relation to sewage which typhus has not. It is not even the shadow of a reason for or against the point at issue. It would be easy to multiply instances which are quite inexplicable by Dr. Budd's theory. Of these the following is

an example:—During the summer of 1862, only two cases of fever came under my care. The first of the two was that of a young married lady, who occupied a house from which I had removed the preceding May. There was a bedroom in the ground-flat of the house, separated from the water-closet by a stone wall. Some defect in the construction of the sewer permitted a communication with the flooring of the bedroom. The consequence was a decided odour, which in damp weather became very strong. For this reason I had, while living in the house, caused the furniture to be removed from this room, and kept it shut up. I warned my successors of the danger of using it, but, on account of its other conveniences, my remonstrances were disregarded. In six weeks the lady took typhoid fever, and swam for her life through a long illness.

No possible source of infection could be traced. No others of the family took ill, the greatest precautions being afterwards used to avoid the source of danger. The house was a new one, and as I happen to know all the tenants who have occupied it, I am able to state that there had never been, up till that time, a case of fever in it. When this lady took ill there was no case of fever in or near Penicuik.

I shall relate one other instance. In 1863, simultaneously with the outbreak of fever in Penicuik, a number of cases occurred in the Wellington Reformatory Institution, situated on rising ground about two miles south from Penicuik. At that time the buildings consisted of an old two-storied house in the centre, inhabited by the superintendent and his family, and a new building joining it at right angles, in which were two dormitories for about ninety boys. Adjoining, and in a line with the other gable, was a cottage in which the grieve or farm superintendent and his family dwelt. The first case that occurred was in the grieve's family. His daughter, a girl of sixteen, was a domestic servant in a family in Penicuik, but visited her father's house regularly. She was quite out of the way of any infection while in Penicuik. While on one of her visits to her father she took ill. *At the same time* several of the boys belonging to the Institution began to droop; and within six days more, sixteen of the boys, two others of the grieve's family, the superintendent's wife, and two children, had taken ill. There were no deaths.

There were two distinct sources of disease, each of which was sufficient to account for the outbreak. The superintendent's wife informed me after her recovery that she had been for months conscious of a bad smell about the house, which was always worst in damp weather. Accordingly, the flooring was lifted, and it was discovered that in and around the house there were old cesspools which had been full for a period of many years during which the house had been uninhabited.

The other source was the cesspools belonging to the boys' privies. These overflowed into a ditch, between which and the Institution there was only the breadth of the highroad. In passing along this road, the smell in hot weather was quite perceptible. There was no possibility of infection from the fæces of a fever patient in this case.

That alvine discharges from a fever patient should be as active, or even more so, in originating the fever poison as ordinary sewage, it is very reasonable to suppose. But facts are quite against the supposition that they are always or even frequently the source. Were such the case, an epidemic of typhoid fever should become more virulent the longer it lasted, because the source of the poison would daily become more abundant. Whereas, at least in my experience, the contrary is the case; a number of cases always breaking out at the same time, the subsequent cases being less numerous and often milder. Besides, the theory breaks down altogether when applied to the numerous instances in which persons take this fever who have never been in any possible way exposed to such a source. Though the exciting cause of fever is, in very many instances, connected with sewage, I think those who limit it to that are in error. It is highly probable that other kinds of organic matter in a state of decomposition can originate the poison, though not so readily. The case of the child who took fever from the foul air of a window-sink is, I think, in point. Neither urine nor fæces were ever put down, the smell which enabled me to prophesy fever probably arising from the decomposed food which was carried into it by the dish-water.

I think the case of the stinking drain which infected so many families in the street *a* bears in the same way. The filth of which that drain was the bearer consisted chiefly of the excrementitious discharges of cows and pigs. Yet I consider that the probability of that drain being the cause of the fever in its neighbourhood is almost as near certainty as such a thing can be. It was the only distinguishing sanitary condition between the affected houses and others in their immediate vicinity. Compare them, for instance, with those on the other side of the street. The inhabitants drank the same water, bought their provisions in the same shops, and were as well housed and clad.

It was not due to infection, for there was quite as much intercommunication with the opposite side of the street as there was with the houses adjacent. None of them had water-closets, so that it could not be a choked sewer or a poisoned drain. The circumstances tie us down to discover a local cause, and no other than the one I have given can be found at all proportionate to the effect.

Two things seem to me to have a strong analogy to typhoid fever.

Students and others who are much in dissecting rooms are liable, especially if from any cause they become weak, to a slight feverish attack, which generally passes off in a few days. The symptoms are depression of spirits, loss of appetite, a slight amount of fever, and diarrhœa, with considerable flatulence. The gas passed by the bowels is fœtid, and can often be distinguished as having the special odour of the subject which the student has been dissecting. Clearly, in this case, the gaseous results of decomposition, after circulating in the blood, are thrown off by the bowels, exciting general and local irritation in their passage. It is highly probable that typhoid fever owes its local symptoms to the inflammation of the glands, caused by the passage of some similar but more virulent poison.

Epidemic cholera is another instance in which a poison produces local bowel symptoms. The causes of cholera and typhoid fever are identical, the different results being the special influence of climate. The epidemics of cholera, which have ravaged Shanghae for several summers, were first produced by the influx of country people, who fled for protection from the Taepings. They arrived in multitudes which far exceeded the house accommodation of the native part of the settlement. The houses, which they rented by the month, were each packed by about a hundred persons, and when they were turned out at the month's end, the floors were often covered with from one to two feet of excrement.

Excluding cases in which the disease may be conveyed by the patient's stools, the question as to whether it can be communicated by exposure to influences arising from contact or proximity to the persons of the sick, is still undecided. That it is not so contagious as smallpox or typhus is abundantly evident from the way in which it selects certain houses, or sets of houses, and passes over others. That it can originate without contagion can be shown in many instances in which solitary cases of the disease spring up when there is not another within many miles. But that it is infectious in a certain degree, and especially in certain conditions, I firmly believe. The circumstances of the case render this very difficult of proof. It is evident that before any person can be exposed to contagion from the person of a fever patient, he must be for a longer or shorter space of time in the house in which the patient resides. If fever follows the visit, you cannot prove to your opponent that it was a case of infection, for he immediately replies that the same original sanitary defect that produced the first case produced the second. The possibility of this objection being urged prevents my quoting many instances which I believe to have been due to personal contact with the sick, but three cases which seem to me to exclude it are, I think, worthy of being recorded.

The first is the case whose history I have given above; the second that occurred in 1863. The husband, who was the first to take ill in that family, had never been exposed to any source of the disease as far as was known, except the bed-clothes brought by his wife from the cottage in which the boy lay who first took ill. There is a considerable distance between the two cottages, and the stools from the boy were emptied on a dunghill close to the house.

The second case was that of a young man who went several times to see his sweetheart who was lying ill, and caught the disease from her. She had been sent home from her master's house, where she had been seized, along with one of his children. As there were neither privy nor watercloset in the cottage in which she lived, and as there was no fever where the young man lodged, I have no doubt that the infection which seized him was due to the ordinary exhalations from her person. His case was slight.

The third instance which I shall give is a very singular one.

In 1861, there lived near Penicuik, in a large new house, with perfect drainage, a boy aged about twelve years, whom, for the sake of reference, I shall name A. A. had a cousin of about the same age, who had come from Edinburgh to spend a few days. The children of an officer in Greenlaw Barracks, which are near the house, were sent to spend the day with A. and his cousin. Three soldiers were sent to erect a small tent for the amusement of the boys. The following day A. was indisposed, and in two days afterwards took to bed ill of fever, from which he died, after a long illness. A.'s cousin took ill in Edinburgh some days afterwards, and recovered, after passing through a very decided course of the disease. The three soldiers took ill, and one of them died.

There were no previous cases of fever among the soldiers at Greenlaw. The nearest case of fever at the time was in Penicuik, where it was very prevalent. A. had been several times in Penicuik, and the only possible explanation I can give of so many people catching fever at one fortuitous interview is, that A., who was the first to take ill, had the infection about his person. The turf on which the tent was pitched was removed from the influence of drainage of any sort.

A young lady who called at the house on the first day that A. was in bed, finding no one at home at the time, spent an hour in his bedroom. She took ill of fever that day week. There was nothing wrong with the drainage of the house in which she lived.

These and similar cases have confirmed me in the belief that whether we are quite certain of the personal contagion of this disease or not, the only safety, practically, consists in acting

rigidly as if we had decided in the affirmative. I think it highly probable, that persons living in conditions which favour the access of this fever, are much more apt than others to become infected by contagion, while, conversely, those living in a pure air, while more able to resist contagion, are more amenable to the influence of the poisonous gases. This opinion is, I grant, very speculative, and I apply it exclusively to young people, who are the most frequent subjects of the disease.

It seems to me to be certain, that typhoid fever is the product of several very distinct causes.

First, there must be a susceptibility on the part of the patient. The way some persons withstand constant exposure to the disease, without taking it once, shows this very clearly. This susceptibility scarcely exists after the age of thirty.

Secondly, a cause which is most tangible, and which has therefore been most talked about, is the emanations from decomposing organic matter, nearly always sewage. This has been shown over and over again. To deny it shows either ignorance or prejudice.

Thirdly, heat and stagnation of the atmosphere nearly always exist, though not necessarily.

Fourthly, a thing or things unknown, probably due to a condition of the atmosphere. To this cause must be attributed the varying cycles of the disease, the preceding conditions being much more constant than the prevalence of fever has been.

It would be interesting, but fruitless, to speculate as to how many of these causes must co-operate in its production; whether, if some of them exist in a high degree of force, a less degree of the other or others suffices; or whether, in a still more exaggerated state of energy of one or more of them, the others may, in exceptional cases, be absent. It will be productive of more benefit if medical men will set themselves to the removal of the source of disease which is within their power—filth in all its forms.—*Edinburgh Medical Journal, Jan.* 1865, *p.* 599.

4.—ON THE USE OF STIMULANTS IN FEVER.

By Harry Leach, Esq., Resident Medical Officer to the Hospital Ship Dreadnought.

[Typhoid is the prevailing form of fever received on board the Dreadnought Hospital Ship.]

Of the sixty cases admitted during the past year, wine or brandy was prescribed for thirty-five patients, whereas the remaining twenty-five received no stimulants whatever. Statistics in medicine are so often fallacious, that these figures merely go to prove that the latter were mild, and the former severe cases

of typhoid. But the principle of waiting and watching for "the time when" to give stimulants in fever appears to be superlatively worthy of attention. Thus, none of these patients commenced wine or brandy before the eighth or tenth day of the disease, and, in more than three-fourths of these cases, until the fourteenth or fifteenth day. The quantity given in twenty-four hours averaged six ounces, and never exceeded twelve ounces, this latter quantity being sometimes judged necessary for a period of from forty-eight to seventy-two hours at about the eighteenth or twentieth day of the fever.

Four deaths occurred—severally from perforation, double pneumonia, exhaustion, and exhaustion with extensive ulceration of the larynx.—*Lancet, Feb.* 4, 1865, *p.* 124.

5.—ON THE TREATMENT OF MALARIOUS FEVER.

By E. Hare, Esq., Deputy-Inspector-General of Hospitals, Bengal.

My arrangements for the experiment on fever were to give quinine a full trial as an antidote. I gave it, therefore, irrespective of all variations of symptoms, except that the more violent and more dangerous these symptoms appeared the larger, earlier, and more frequent was the dose of quinine given. My assistant was directed to mix a large bottleful of quinine, rubbed up in plain thick mucilage, ℈j. to the ounce. I would not give it in its more soluble form with diluted sulphuric acid because I feared that in the frequent and large doses I proposed giving, the quantity of sulphuric acid required to dissolve the quinine being considerable, it might injure the stomach; and besides, being an experiment, which I wished to be simple, and free from all objection, I preferred giving the quinine pure. One dose of a purgative, viz., ℥j. of compound jalap powder was allowed to be given in some cases, but latterly, especially during the rains, the purgative was very rarely given. All the worst cases, in fact, had no purgative, for I was too anxious to give as much quinine as I could, and as early as possible, and was afraid of losing time by nauseating the patient with other medicines. Moreover, I believe the purgatives delay and prevent the action of quinine, and remove it from the system too rapidly. Sydenham and many other Practitioners have observed "that we are in danger of bringing back intermittent fevers if we employ purgative medicines too soon after we have stopped them by bark," and we have the same observation in De Haen ('Cullen,' vol. i., p. 642.)"

The patients came in at uncertain hours, and I could not be

always in the wards. I therefore gave a standing order to my assistants that the moment a patient was admitted he was to have ℈j. of quinine. I saw him myself always a short time afterwards, and gave him another ℈j. or ℥ss., according to the urgency of the symptoms. In visiting the patients an assistant accompanied me, carrying the large bottle of quinine described above and a two-ounce measure glass, and each patient had an ounce = ℈j. of quinine given to him, and it was swallowed before me. Ordinary cases took this dose three times a-day, but if the fever was dangerous I gave ℥jss. of the solution, or ℥ss. of quinine at once, and ℈j. every three or four hours afterwards, according as I found the stomach would bear it without vomiting. It was very rarely necessary to continue this treatment beyond the second day after admission. The patient was almost invariably convalescent on the third day, when gr. v. doses three or four times a-day for about two days more, with good food, completed his cure. Throughout the treatment, if the patient had had his fever long, and thus, being without appetite, had taken no food for some days, I gave him moderate quantities of fluid nourishment without delay—as arrowroot and milk, and soup of moderate strength—and if he were a man of free, or especially of intemperate habits, I allowed him, if he had been ill a few days and without his usual stimulant, a small quantity of brandy or wine.

I may safely remark that in the annals of Medicine such an experiment as the above, in simplicity and decisive results, has never before been recorded. If the table of Mortality No. 3 of Non-Military General Hospital be compared with the Military (No. 4 Table), which forms a part of the same range of buildings, the difference will be seen to be very great. In the non-military Hospital, during the twenty previous years, there was 1 death in every $11\frac{1}{4}$ admissions for fever, whereas in the military the mortality was only 1 in 30, and the cause of this difference will be at once perceived, from the very different description of cases admitted in each. The military patients are simply the soldiers of the garrison of Fort William, who are sent to Hospital on the first appearance of sickness, whereas the cases received into the non-military wards are from the unemployed sailors wandering about Calcutta—in fact, any European who is sick and has no other home to shelter him. Sailors and others are often thus picked up from the roads of Calcutta in an insensible state and sent to the Hospital. They are, therefore, the most hopeless class of cases that can be treated in the deadly climate of Bengal, and their treatment having been so long delayed, becomes most difficult. The large mortality—1 in every 11 cases of admission for fever—for the twenty years before my experiment

proves how severe the cases must have been which I treated there. The mortality also for dysentery—1 death in every 4½ cases—during the same period (see the Table No. 3) is another instance of the enormous mortality which had been habitual from the above cause in the non-military Hospital. My treatment, therefore, comprised the young, robust soldier, treated immediately after his sickness appeared, and the debauched, drunken outcast from the streets and ditches round Calcutta, where they were frequently found by the police after lying there all night.

I will now, having explained what the experiment was, state the deductions which I think must be drawn from the result.

In the first place, no blood was drawn either by lancet or leeches. Bleeding, therefore is not necessary, and the disease not inflammatory. No opium; no purgatives to bring away bad secretions; no drug of any kind is required, except quinine, for the successful treatment of malarious fever.

Quinine may also be given in the largest doses, whether there are head symptoms, delirium, coma, or pain in the liver. Whether it be in the hot stage or cold, quinine is not only safe for all forms of malarious fever, but its certain cure; and in cases where there is danger to life, the earlier and the larger the doses of quinine which can be given to the patient the better. Vomiting and full cinchonism were my only checks for continuing it in full doses. I mean by cinchonism the peculiar head symptoms which large doses of quinine produce, and when they were fully obtained I considered my patient safe, and that only moderate doses were then required. I always waited, too, till the cinchonism had lessened before I continued even small doses—gr. iv. to v. three to four times a day for about two days longer.

Quinine, therefore, may with reason be pronounced as a direct antidote to the poison of malaria, and not simply as an anti-periodic and adapted only to stop periodicity, for it always cured equally well those fevers in which there were no periods, but which continued without the slightest remission during the twenty-four hours.

I must here also explain a deduction from my experiment which I had long meditated before I went to Calcutta. It had constantly struck me that all the fevers I met with in India were caused by malaria. All infectious fevers, as typhus and typhoid, are unknown in the plains of India among the European part of the population. I have met with only two cases of typhoid fever, with ulceration of Peyer's glands, during my practice in India, and they were both recent arrivals from on board ship, and these cases I saw after my Calcutta experiment. I therefore had come to the conclusion that,

with rare exceptions, all fevers in India, whether called ardent, continued, congestive, inflammatory congestive, remittent, or intermittent, were all caused by one only cause, viz., the blood poison malaria. It was my conviction of this truth which justified my indiscriminate and determined dosing with quinine every fever patient admitted into my wards, and I was guided a good deal in my plans by the following passage from Dr. Holland's "Notes and Reflections," p. 150 :—

"And as bark by curing associates together many intermittent affections, under the presumption of a common cause, so may colchicum furnish similar inference regarding affections seemingly remote in situation and symptoms. This mode of inference, hitherto very limited, may hereafter be extended much further. It is one of singular value, as closely connecting the treatment with the theory of the disease, and rendering them mutually corrective of each other. It at once enlarges and simplifies our views in every part of pathology; each fact well ascertained is fruitful of results, far beyond those which directly appear to the view."

Again, p. 348, he enumerates these as proofs of a disease having a common origin with ague, viz., malaria :—

"1st. By their intermissions.

"2nd. By their succession to, or alternation with, intermittent fever.

"3rd. By the influence of the same specific remedy in removing or relieving such disorders."

He says the application of these proofs might be largely made.

Again, p. 354 :—"The influence of certain medicines, and particularly bark, in curing even the most anomalous varieties of these intermittent disorders, is a fact of great interest. Like the use of mercury in obscure syphilitic affections, or colchicum in the most irregular forms of gout, it enables us to denote and class together symptoms apparently the most remote in kind, but which presumably could not thus be relieved unless depending on some common cause. We have, therefore, in the specific nature of the remedy a sort of practical test of the character of the disease, often of great importance to the consistency and success of our treatment, and related, through the principle of the inquiry, to some of the most interesting questions in all pathology. This it is, as was noticed when speaking elsewhere of colchicum, which gives peculiar value to all which illustrates the action of these remedies. They are the interpreters of facts, far beyond their momentary effects, and of connections between morbid states, which are in no way so definitely made known to us."

The success of this method is acknowledged in establishing

the gouty nature of many diseases of the joints, eye, heart, &c., by colchicum; by mercury, in testing the syphilitic nature of many diseases of the skin, iritis, &c.; and by quinine and arsenic, in proving many neuralgias and other very numerous chronic diseases, described by MacCulloch, to arise from malaria. But I believe that I am the first who ever thought of applying the principle, connected as it is with some of the most interesting questions in all pathology to acute disease, viz., all the fevers of India. And let me remark that colchicum and mercury being purgative, and acting freely on all the secretions, may act thus beneficially on all diseases apart from any specific effect; but this cannot be said of quinine, which acts neither on the bowels nor any of the secretions. This reasoning, therefore, applied to quinine may be received with greater certainty than to the others; and yet remarkable and valuable results have been obtained from experiments on the latter. It may, therefore, be received with the greatest confidence that the fact that 421 fever cases, indiscriminately admitted during all the seasons of the year, were, I may say, saturated within two days with quinine, with only three deaths, in two Hospitals in Calcutta, where the usual mortality had been, for twenty years previously, one death in eleven cases, and one death in thirty, proves beyond dispute, to my mind, that all these fevers were of a malarious origin. For, mark, it was not only that quinine was given with impunity—that is, that it did not increase the average mortality, which had been, before its use, enormous.

In reviewing the results of quinine in the treatment of malarious fever, a necessary question arises, How does quinine act? It was introduced to the world as a tonic, and it produces very remarkable head symptoms. So long, therefore, as the congestions of fever were considered as inflammatory, and remittent fever to require active depletion, we need scarcely wonder that it should have been so carefully excluded, especially when the fever caused head symptoms; and it was this prejudice in particular which I especially went to Calcutta to disprove, for I knew that these latter cases were the most fatal, and therefore those requiring the earliest and largest doses of quinine, and the head cases were those which, in my experiment, I especially attacked vigorously with my remedy; and it was soon evident to me, as it will be to any one who tries the large doses, that, so far from exciting, quinine produces the most deadly coldness of the skin, feeble pulse, sometimes intermitting, complete deafness and amaurosis, evidently from its powerfully sedative, almost paralysing, action on the brain. The delirium also it causes is not active, but a low muttering wandering. It is, in fact, to me quite certain that no one had ever tried quinine in

acute fever except in small doses, when it does produce often a peculiar excitement, especially of the head; but in large full doses it is the most powerful and alarming sedative. I have during my practice often been frightened with my own results. I recollect one case which made a strong impression on me. It was in Burmah. A young officer of my regiment, Lieutenant M., was sent in from an outpost, having had remittent fever for some days, and he was reported to me as having been delirious the night before, and in great danger. I saw him first about 3 p.m., and found him in the most hopeless state. The accession was just coming on, and I knew that he must die in the night if it was not checked. There was therefore no time for delay.

I instantly gave him ℥ss.; at 5 p.m. he took ℈j., and at 7 p.m. ℈j. more, or seventy grains of quinine in four hours, and I sat down by his bedside to watch the result. No paroxysm appeared —it was quenched by quinine as water does fire. His skin became deadly cold, his pulse fluttering, with complete deafness and muttering delirium. He was very restless, complaining that he saw devils and things floating before his eyes. The pulse alarmed me greatly, and I gave him repeatedly a glass of port wine. In the course, however, of about four hours these dangerous symptoms subsided. He fell asleep, and the next day was not employed in giving him medicine, but in giving him food. His fever left him from that night, and he was soon well, and is now strong and robust, and an ornament to his profession. Major S., of the same regiment, was almost a similar case, and I could enumerate numerous others. No one in fact, living, has given quinine more largely than I have, and though I have as yet had no fatal result from it, I have often dreaded such a misfortune, in cases where I felt justified in giving it in very large doses; and the dangerous symptoms have always been, not excitement, but deadly coldness, arrest of the pulse, and paralysis of the heart. These, however, are the effects of large doses; given in small quantities it produces, in some cases, a peculiar kind of excitement.

I will now close my account of the Calcutta experiment. Its results were published to my service in India by a copy of the report on it being sent by the Medical Board to every Medical officer. I was, at my own request, posted to an European regiment, and I determined quietly to allow the seed I had sown to produce its fruit, and as for myself, with the noble opportunity which the Surgeon of a European regiment has, especially on service in Burmah, I resolved quietly, instead of perpetually writing, to wait my time, collect my results, and see what further experience would teach me.

I have since treated 6982 fever patients, with only one death

in every 211¼ cases, and I have to describe but one change in my treatment, which, however, I consider a most valuable and important improvement. I have, in fact, gone backward, it may be thought by some, in my antispoliative treatment, and resort now, I am certain in some cases with great advantage, to mercury.

I have not, however, lost any of my old confidence in quinine. I maintain still that given early and in sufficient doses, it is a perfect antidote; and I would cheerfully go back again to Calcutta and try my experiment again with quinine *only*, and a syringe to wash out the bowels with plain water. With water to wash out the bowels and apply to the head, and plenty of quinine, I want no other medicine for the treatment of the worst forms of tropical fever. Leeches, bleeding, purgatives, emetics, are, if not useless, at any rate unnecessary. You can cure all your patients without one of them. Why, then, do I now use calomel? I will explain.

Quinine, as is shown above, when given in the large doses which are occasionally necessary to save life, in cases where the fever has been of some days' continuance, produces very dangerous and distressing symptoms; though I assert again that I have never yet had a case where the results were fatal, nor do I know any case of mine in which even the patient complained of any lasting bad result from it, and in a regiment I was with so many years I must have known these results, if there had been any. Yet I can easily conceive of such accidents occurring, where very large doses, as ℥j., were given at once, but with only one ℥ss. dose to commence with, and ℈j. doses given at intervals of never less than three hours (and I have rarely, indeed, found more necessary), the symptoms of cinchonism will soon warn you to stop the medicine before sufficient can have been given to poison.

However, besides this, any one who treats many bad forms of fever will often find himself puzzled how to act when the patient, as he often will, vomits his medicine. It may be written down, as an axiom in all cases true, give plenty of quinine to a case of malarious fever and it will be cured; but this is not always so easy in practice as it appears on paper, for if you cannot give the quinine because the patient vomits it, what are you to do? You are reduced to the same perplexity as Dr. Johnson was in his first trial of bark. With the very bad and long-neglected cases in the non-military General Hospital this was the bane of all my pleasure, for I really would have enjoyed my practice there but for the anxiety these vomiting patients sometimes caused me. When I went into the ward and asked how Smith or Jones was, the apothecary would tell me he was very bad, and had brought up all his medicine. There was no time for

delay. The patient, if he could not retain the quinine, would probably die in the night; and I would go to his bedside, and persuade him, perhaps with difficulty, to swallow another dose, but before I left the ward, to my disgust, I heard the sound of vomiting again, till sometimes I was much perplexed. However, by not allowing the patient to drink anything, and by giving the quinine in pills, in port wine, in soup, or, if the patient were a spirit drinker, in brandy, I always contrived to give him sufficient to check the fever, and then his stomach would bear more; but still it was only by very careful management that I did so, and always with great discomfort and distress to the patient. In the General Hospital I had no choice. I was obliged there to continue the simple quinine or give up my experiment: but since then, while I was in Burmah with my regiment, and was trying there calomel and quinine in large doses in the treatment of malarious dysentery, I soon became convinced how much one drug assists the other in producing its peculiar constitutional effects. There are many cases of fever which I described above which cannot be salivated with the largest doses of calomel aided by mercurial frictions, but if you add quinine to these doses you will find that salivation quickly appears.

Dr. Morehead notices the same fact, p. 205. He says:—"I can state from my own observation that it is by no means an unusual occurrence in the course of remittent fevers, which in their early stages have required the exhibition of several full doses of calomel, to observe, after the recurrence of the fever has been prevented by quinine, a slight mercurial action to appear on the second and third day, though not more than a few grains of calomel or blue pill, in combination with quinine, had been given during those days." Dr. Watson also, and many other writers on the treatment of intermittent fever, notice how much better and more quickly quinine acts the next day if a full dose of calomel be given overnight.

Again, when I left my Hospital, and had to treat children for fever, I found the little things sometimes obstinately refuse to take large doses of the bitter medicine, and often if they did take it, it was vomited; and I confess that I rarely now treat a young child for bad fever without mixing calomel with the quinine. Its effects on them, without producing any visible constitutional effects, are marvellous, and the quantity of quinine you are obliged to give is greatly reduced. My rule, therefore, now is, if a patient can take quinine in the quantity I think necessary for his safety, I give it only; but if he vomits it often, I give him a full dose or two of calomel, and I prefer the old ℈j. dose to any other from its inimitable effects in checking vomiting. I never have had occasion to exceed two

doses; and I find I can then continue the quinine without any trouble to myself or patient. The quinine also need not be stopped, but given with the calomel; the calomel put dry on the tongue, and the quinine given in pills.

To a child I give one large dose of quinine to start with, and try and repeat it, but if the little thing is sick and refuses it, I drop two or three grains of calomel dry on its tongue, and mixing three to five grains of quinine in jelly, I make it into a pill, and put it on the end of my finger into the child's throat. This cannot so easily be done with ten grains, which without the calomel would be required. Very soon, however, you will find that this treatment will check the fever, and you can then stop the calomel, and continue only the quinine. There is no necessity to salivate any one, or give a grain of calomel more than just sufficient to assist the quinine, and in many cases it will be found a great comfort to both patient and physician to give it in this way. I believe myself that as quinine is an antidote so is calomel, and that the two in combination have more powerful and immediate effect than separately. Were we to be so unfortunate as to lose cinchona, which to us residents in the tropics would be as sad a loss as the fire which Prometheus restored, we must resort, I think, to the calomel system again, for bleeding alone and purgatives will not check the paroxysms of remittent fever. It requires, as Johnson soon found, something more to propel the stagnant congestion, and the salivation system only began to yield as quinine was gradually introduced. In the chronic forms of malarious disease, where we have time to produce the constitutional effects of arsenic by small doses, we could use it, for the results of arsenic are often in ague far more permanent than quinine.

In conclusion, also, I must, in self-defence, explain other details in my practice. My published treatment may induce my readers to suppose that I treat every case of fever with ℈ss. and ℈j. doses of quinine, and push it to the utmost; but it must be recollected that my published cases, and the treatment I have recommended, is only for *extreme* cases, and where there is urgent danger of life. Also I must be pardoned, in performing an experiment on quinine in the General Hospital, for pushing my treatment to extremes. Half the value of the results of my experiment would have been lost, if I had not done so, for it is only the extremes of any practice which show its really hurtful tendencies. And I confess that I might have saved much of the quinine which I gave to my patients during my trial; but I might have lost some of them had I tried to adopt this economy. I never witnessed any harm from giving over-doses; and I thought it right, to settle finally the disputes about the danger of quinine in head symptoms and local con-

gestions, to push it to extremes; and no one, I think, can be sorry that I did so—not even my patients: but I do not do so now. I do not, moreover, push quinine through the paroxysm, if I have time, and can safely avoid it; and prefer the sweating stage to any other for my largest dose, for the febrile disturbance of the accession is unfavourable for the absorption of all medicines—quinine, calomel, and purgatives included. All I say is, if there is danger, do so. Only a few cases also require heroic doses, but these being more particularly described, assume the most prominent position in every description of systems of treatment, and are too apt to be considered as resorted to in every case. I frequently give fever cases only ten-grain doses, oftener still only five grains three or four times a-day, and to a few none at all, but treat them with a purgative, low diet, and rest in bed. In justice, however, to your patients, you ought to give them a little quinine, for why let them be sick, when they may be well so much sooner? But when I see a patient with dull eye, cold clammy skin, tremulous tongue and oppressed pulse: or another, with acute head symptoms, delirium, and impending coma; or a child, with spasmodic contraction of the thumbs, squinting, or actual convulsions, in a paroxysm, I do not lose time by preparing such cases for quinine with purgatives, &c. I know the danger impending, and I give them (at least the adults) ℈ss. of quinine at once and follow it up with ℈j. doses every three or four hours, according to the symptoms it produces, till they are fairly cinchonised, and if they vomit the quinine I add ℈j. or ℈ij. of calomel. I never give more than ℈ss. of quinine in one dose, and prefer frequently repeated ℈j. doses, because you can watch better the symptoms it produces, and stop it before the cinchonism becomes extreme.

Also, with respect to diet, I do not offer or prescribe food for a young soldier just attacked with fever; I would give little or nothing for the first day or two; but if I have a patient sent me who has had fever for a week or more, I feel sure that he is starving, although he may have no appetite—perhaps, even he may loathe food, but I persistently make him swallow small quantities of soup, arrowroot, and milk, and if I know him to be a spirit drinker, or even a free liver, I give him wine or spirit. This is all I wish to impress on my readers: that malarious fever is not an inflammatory disease, but the direct contrary, and requires no depletion by bleeding or purgatives, and that the congestion of the liver, &c., which appear in fever, and the morbid secretions thence arising, are best cured by early and large doses of quinine, under the use of which the congestions disperse, and with them the morbid secretions. Also that we can support the strength of the patient without danger by

proper food and nourishment. Let a patient be therefore the first day or two of his fever with but little food, but afterwards give it to him in moderation, and especially do not starve children.—*Medical Times and Gazette, Nov.* 26, *and Dec.* 17, 1864, *pp.* 563, 647.

6.—REMARKS ON SCARLATINA.

By Dr. William J. Cummins, Cork.

[Dr. Cummins has recently had many opportunities of testing the treatment of scarlatina by sulphites, so much advocated by Prof. Polli of Milan, and Dr. de Ricci (see *Retrospect*, vol. 50, p. 23). The salt used was the bisulphite of soda in ten grain doses every one, two, or three hours according to the case. It will not produce any marked effects when the rash is already well out, for the catalytic action has then already taken place.]

What, it may be asked, is the use of administering such a medicine? I answer, because experience has taught us that the chief danger in the malignant variety of scarlatina (into which any of the others may lapse) is the tendency to secondary inoculation of the blood, a monster evil, which the sulphurous acid, set free when a bi-sulphite is decomposed by the acid of of the stomach, tends to prevent. But, apart from the use of this medicine during the course of scarlatina, it is valuable as a prophylactic.

I have already mentioned that, before I was able to procure a supply of bi-sulphite of soda from Dublin, thirteen members of four families were attacked by scarlatina, and that three died (one more died a month after attack, from asthenia, caused by an abscess in the neck); it has also been stated that, when subsequently using the medicine, fifteen cases occurred in fourteen families, with no death directly due to the disease in any, except the two cases already detailed, in which the medicine was tried as an experiment *late in the disease*, when the patients were at the point of death. But I have yet to mention that the other members of these fourteen families, as well as of one other which had lost a child in twenty-four hours, without medical aid—in all fifteen families, numbering thirty-seven individuals—took, by my orders, ten grains of bi-sulphite of soda twice a-day.* This accounts for the comparative immunity which they enjoyed from the effects of contagion; for, with the exception of three who where attacked by the precursory symptoms of scarlatina—such as vomiting, sore throat, and transient eruption, which,

* The use of bi-sulphite of soda as a prophylactic for zymotic diseases has not been limited to scarlatina, the author having prescribed it during the last few weeks for several families exposed to the contagion of typhus, and so far with success.

on pressing the medicine every three hours, subsided entirely by the following day—and three others who went through a brief and modified attack of scarlatina, they all escaped the effects of contagion to which they were exposed in close unventilated houses. The results of the trial which I have given the bi-sulphite theory, tabulated, are as follows:—

Taking the medicine late in the disease by cases at the point of death (in one only for a few hours), 2; died 2.

Without the medicine.			Taking the medicine prescribed early in the attack.		
Families	Attacked	Died	Families	Attacked	Died
4	13	3	14	15	0
Besides one death indirectly caused by scarlatina a month after attack.			One death occurred indirectly from scarlatina a month after attack.		

Using the medicine as a prophylactic, by persons exposed to contagion in their own houses.

Individuals	Attacked		Cut short	Modified	Escaped entirely
37	6	Result, . .	3	3	31
		No death.			

The very extreme of the importance of results such as these tends to cast a doubt on their reality; and, while I can assert that I have not overstated them in any particular, I must add that I accept the conclusions to be drawn from them with reserve, and look anxiously for their confirmation in any trial which others may be induced to give the medicine on my recommendation.

Another medicine which has been used for its direct effect upon the blood in scarlatina is the chlorate of potash, and I can speak most favourably of its use. Given in doses of from five to fifteen or twenty grains, according to age, it produces a manifest effect upon the condition of the throat and the colour of the rash, which is perhaps due to the oxygen it contains; but the chlorine itself may act as a disinfectant, or when urea is retained and decomposed in the blood into carbonate of ammonia, by combining with the latter and forming a chloride of ammonium. This medicine has often failed, however, and occasionally seems to produce no effect whatever. I cannot speak from experience of the sesquicarbonate of ammonia, of

chlorine water, or of other medicines which have been used for their direct effect upon the blood in scarlatina; but the muriated tincture of iron is a remedy of great value in the malignant form, especially when there is diphtheritic exudation or a decided erysipelatous tendency.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 23.

7.—THE BLISTER TREATMENT OF RHEUMATIC FEVER.

Cases under the care of Dr. Jeaffreson, at St. Bartholomew's Hospital.

[See last volume of the *Retrospect*, page xi. of Synopsis, for a brief epitome of this plan of treatment.]

The safety of the heart is undoubtedly the main point at which every treatment must be directed; and in this particular more especially does the Blister Treatment exhibit its peculiar value. In a communication read Wednesday, March 22, by Dr. Davies at the Hunterian Society, he stated that of 50 cases which had been admitted under his care at the London Hospital 27 had hearts already damaged by recent or old inflammatory mischief, and 23 were free from cardiac complication. The results of the blister treatment in these fifty cases showed that as many as twenty-five, when discharged from the Hospital, were totally free from any endo- or peri-cardiac disease; or, in other words, that while every heart was saved which came in sound, two recent cases of endo-carditis were apparently cured by the alteration effected, as he believed, in the alkalinity of the blood by the free discharge of serum from the neighbourhood of the inflamed joints. Dr. Davies also states that those cases answer best to the treatment in which a great number of joints are simultaneously affected, and when, by setting up a large amount of discharging surface in the proximity of the inflamed parts, a large proportion of the *materies morbi* may be evacuated at one *coup*. Cases where the poison would appear to crop up to the surface by instalments, attacking the various joints at intervals of days, do not afford such striking examples of the efficacy of the treatment. The first case, where an unexampled amount of blister was applied in an extremely acute case, and where the patient was discharged cured in thirteen days, will well illustrate Dr. Davies' position. That this treatment is not simply local in its action was also shown in the alteration produced in the urine in the majority of the cases cited; for in 11 the urine remained acid, but generally diminished in acidity during the whole period of the case; in 22 it became neutral shortly after the serum was discharged; in 10 it exhibited an absolute alkaline reaction; while in 7 no notes were taken.

Case 1.—William S., aged thirty-two, a working silversmith, and exposed to great variations of temperature, was admitted into the Hospital on December 2, the seventh day of his illness, and was discharged cured on December 15, thirteen days after he came under treatment. Eleven blisters, amounting to 482 square inches were applied simultaneously, and with almost immediate relief. As the patient said, "the rheumatic pains left me as soon as the blisters drew:" and on the third day from admission all pain had disappeared. The pulse fell from 105 to 95 per minute: the temperature from 101·4 to 99·6 and 98·8; no cardiac mischief was developed. The urine, scanty and acid on admission, was rendered slightly albuminous from the presence in it of a small quantity of blood. The slight strangury and albumen, however, disappeared in forty-eight hours. He had slept very badly from the commencement of his illness, but as soon as the poultices were applied to the blistered surfaces sleep returned, and was "good" every night during the time he remained in the Hospital. His appetite, bad on admission, was good on the third day: and his thirst, which was slight when he came under treatment, was not increased by the blisters, is reported to be absent on the fourth day. The heart was sound when he came under treatment, and free from disease when he left the Hospital.

Case 2.—Wm. P., aged twenty-two, bootmaker, was attacked, for the first time, with rheumatic fever on November 29. He was admitted into the Hospital on December 6, the seventh day of his illness, and was discharged on January 2, 1865. Seven blisters, equal to 133 square inches, were applied on December 9, and the pain, which was severe previous to their application, had disappeared as soon as the surfaces had been dressed. The following day the clinical report states:—"There is no pain anywhere, and with the exception of a little uneasiness on the right shoulder on December 17, he was quite easy and comfortable until the day of his discharge." The pulse fell from 110 on admission to 90 per minute on the third day. The temperature from 101·4 to 98·6. The heart presented a systolic murmur, distinct at admission over base and apex, and was unchanged at the time of the patient's leaving the Hospital. His sleep, which was stated to be bad before the blister application, was improved on the second, good on the third night, and remained satisfactory until he was discharged. The urine presented a slight trace of albumen as the result of the treatment, but was normal and free from that substance forty-eight hours after the blisters had been applied. The appetite, which was indifferent on admission, is reported to have been "good" on the fourth day, while the thirst was absent at first, and continued so, unaffected by the local irritation set up by the blisters.

Case 3.—John B., aged nineteen, was admitted on December 8, 1864, the seventh day of his disease, and discharged January 30, 1865. He had, however, been up and dressed from January 11, 1865. Eight blisters were applied on December 9, three on the 12th, one on the 13th, one on the 14th, one on the 21st, making in all 603½ square inches of empl. lyttæ. The day succeeding the application of the eight blisters the report states that all pain had disappeared. On the fourth day from that date, the patient has "now a little pain in the right shoulder, and says he wants another blister for it," which is applied and followed by great relief. The clinical account says:—"December 14.—The patient has no pain anywhere. December 18.—The same. December 19.—Had pain in the left knee yesterday; I think he caught cold from going to the closet yesterday. And December 20.—The left ankle and knee were swollen. The application of the blister removed all pain and swelling, and never returned during the time he continued under treatment. The pulse fell from 108 to 86, and varied from time to time, with the appearance of the affection in different joints. The temperature fell from 100·8 to 99·4, and also varied with the pulse. The heart presented before the application a systolic murmur at the apex, which remained unaltered at the time of his leaving the ward. His sleep, which has been bad since December 2, was pretty good on the night after the blisters had been dressed; moderate on the fourth; good on the fifth; disturbed considerably by a blister on the 6th; good on the following and every succeeding night. The urine.—Its reaction before the blister application was not noted, but the treatment induced some strangury and traces of albumen, and its condition varied from time to time with the variable condition of the joints. Appetite is stated to have been bad on admission; a little better on the second day from the application of the blisters; improving and gradually becoming good from the fifth day of the treatment. The thirst was slight when the patient entered the Hospital. It appears, however, to have been considerably increased after the application of the blisters, but the symptoms vanished in a few days.

Case 4.—Charles P., aged twenty, butcher, already previously the subject of acute rheumatism, was admitted on December 8, 1864, the fourteenth day of his illness, and discharged on January 18, 1865. Four blisters, equal to 118¾ square inches, were applied simultaneously, and with such relief that on the day succeeding the application all pain had disappeared, and his sleep, which for fourteen days had been wretched and without benefit to him, is reported as being good, and continuing satisfactory for the remainder of the time he was under observation. The pulse, which was 80 and 66 respectively on the two days

before blistering, became 72, 68, 63, and ultimately 60. The temperature, 100·4° and 98·8° before blistering, became 98·8°, 99·2°, 98°, and ultimately, on cure, 98·4°. The heart was free from disease on admission, and continued the same during the entire period. The urine presented after the blistering a very slight trace of albumen. The appetite, which had been bad since November 25, was very much improved on the day after the blisters had been applied. The report says, "he eats everything before him;" and it continued excellent from that date. The thirst was great on admission, only slight after the blisters were applied, and stated to be *nil* on the following day.

Case 5.—Susan J., aged twenty-two, needlewoman, of fair complexion and sanguine temperament, and who had had three previous attacks, was admitted with acute rheumatism on December 12, the fourth day of her illness, and discharged on February 2, 1865. Six blisters were applied: four on December 12, and two on December 14, equal to 328 square inches. On the day succeeding the first application the report states:—"There is no pain now in the hands and ankles, but pain came on in the night in both shoulders, and she has had a very slight pain in passing urine." After the second application the report continues:—"Feels much better; no pain anywhere; got out of bed this morning; wrist still swollen and red, but soft, moveable, and not tender to pressure." On the following day report states:—"She cannot even invent a pain anywhere; looks well, and is only weak. The pulse fell in this case from 114 to 100, 90, and 70, and was 84 at the time she left the Hospital. The temperature fell from 101·7° to 99·7°, or two degrees. The heart was diseased on admission, and the systolic murmur at base and apex remained unchanged. The urine, at first acid, and containing a slight trace of albumen from the blisters, became alkaline on the second day from the time of the local application, and normal speedily after this date. Her sleep, which had been almost absent from December 8, is stated to have somewhat returned after the blisters were removed, to be much better on the fourth, and very good on the fifth night, and to have continued satisfactory until the time she was discharged. Her appetite, none on admission, was improved immediately after the application of the second set of blisters, and pronounced good on the fifth day after the last-named application. The thirst was very great on admission, unaltered the day after the blistering process, much less in amount on the following day, and none on the succeeding days.

[The following case occurred under Dr. Davies' own care at the London Hospital.]

A case at present under the care of Dr. Davies affords an

excellent example of the safe and rapid results of the treatment. The patient was brought into the London Hospital on Friday night completely crippled from acute rheumatism, and presenting the usual symptoms of that disease. He described his agony as very intense. Ten blisters were at once applied, and a grain of morphia was administered internally.

On the Saturday evening he was free from all pain.

On the Sunday he declared himself to be comfortable, and his joints to be moveable.

On the Monday morning he sat up and washed himself, and in the evening was able to walk down the ward to the closet.

On Tuesday he was perfectly easy, with appetite returned, and thirst absent; and was ordered to have a chop and a pint of Bass's ale, as he had been accustomed to live rather freely. The urine in this case was found to be strongly alkaline on reaction, and perfectly free from any trace of albumen as tested by heat and nitric acid. He had had no strangury. On being questioned as to the pain induced by the blisters, he said that he would gladly have them repeated were he unfortunate enough to have another attack of rheumatism. No cardiac mischief was developed. No medicine beyond the morphia already mentioned had been administered.—*Medical Times and Gazette, April* 1, 1865, *p.* 333.

8.—ON THE USE OF AN ALKALINE BATH IN RHEUMATIC FEVER.

By B. G. Everett, Fsq., Ditchling, near Hurstpierpoint.

Having for the past seven years been I might say a martyr to rheumatic fever, very few months passing by (till within the last year) without my being confined to bed, perfectly helpless, for some weeks, I need not say I take a peculiar interest in the treatment of that disease; and having, I quite think, warded off two attacks within the past twelve months, I shall feel obliged if you will insert this in your journal, in the hope that some of your readers will be induced to adopt the treatment in similar cases, and kindly inform me, through your pages, the results.

During a severe attack of rheumatic fever about fifteen months ago, I found nothing relieved the pain or apparently did so much good amongst local applications as spongio-piline soaked in a warm saturated solution of nitrate and carbonate of potash, and applied to the joints; so I determined that when well again, directly I had any rheumatic pains, I would have a warm bath containing potash. As usual, only a few months passed; all the early symptoms of my old enemy returned. I was unable to use my extremities, and also had lumbago, show-

ing there was both muscular and articular rheumatism. I at once decided to try the bath; it contained about twenty-four gallons of water at 100°, two pounds of bicarbonate of potash, and one pound of nitrate of potash. I remained in it ten minutes. The following morning I was much better; there was no fever whatever, and in a few days I was well, and able to attend to my practice.

Shortly after this period I had a patient with rheumatic fever, and it being in its early stage when I first saw him, before the joints were much swollen, I ordered him a similar bath to the one I had, and with a similar result. I also gave it to a little boy, who when I saw him was delirious, although the joints were not much swollen, and there was no muscular rheumatism or cardiac mischief. In less than a week the boy was down stairs.

About a fortnight ago I again had all the premonitory symptoms of acute rheumatism: but at once had the alkaline bath as before. In two or three days I was well (although the knees were stiff and painful for a week), and had not a symptom of fever.

I doubt not I have to thank the bath for saving me from two attacks of rheumatic fever: for until I used the alkaline bath, whenever I had any rheumatism about me, it was sure to run on to the fever, and keep me a prisoner for several weeks.—*Lancet, Jan.* 21, 1865, *p.* 82.

DISEASES OF THE NERVOUS SYSTEM.

9.—ON SCRIVENERS' PALSY, OR THE PARALYSIS OF WRITERS.

By Samuel Solly, Esq., F.R.S., Senior Surgeon to St. Thomas's Hospital.

[This disease must be very rare, for it is not mentioned even in Copland's Medical Dictionary. Cases, however, do occur, and are liable to be mistaken for a symptom of incipient softening of the brain.]

The disease, as the name implies, shows itself outwardly in a palsy of the writing powers. The muscles cease to obey the mandates of the will. It comes on very insidiously, the first indication often being only a painful feeling in the thumb or forefinger of the writing hand, accompanied with some stiffness; these unnatural sensations subsiding during the hours of rest and sleep, to return with the writer's work on the next day.

The loss of power is not sudden, as in a paralytic stroke; nor is it a complete paralysis of any group of muscles. The

paralysed scrivener, though he cannot write, can amuse himself in his garden, can shoot, and cut his meat like a Christian at the dinner table ; indeed he can do almost anything he likes, except earn his daily bread as a scribbler.

Scriveners' palsy is not the only instance of a set of muscles being cramped and paralysed by long-continued exertion. There is, as has been observed by Virchow, shoemakers' cramp, milking cramp, the musicians' cramp, compositors' and the sempstresses' cramp.

When scriveners' palsy first commences, the victim of it only feels its direful influence after a hard day's work. He regards it only as a sign of fatigue, and, as he starts fresh the next morning attaches no importance to it as the first attack of a serious enemy ; but in a short time he is obliged to rest earlier in the day, and hails his early dinner hour with joy, as giving him some respite from the fangs of his tormentor. He tries to overcome his difficulty by holding the pen firmer, but this really only increases the evil. Suddenly he finds his pen dash off at a tangent, and the word that he intended to write in the proper line is, to his horror, commenced in the left-hand corner of the page. Not unfrequently the act of writing is arrested, not by such sudden diversion, but by trembling, and a shaking palsy limited to the right hand.

In the cases that have come under my own notice, I have observed much disturbance also in the function of the nerves of sensation. There is generally a feeling of weariness and slight pins and needles, but usually not numbness. Virchow, who has seen more of these cases, says : "Ordinarily no disturbances of the sensibility show themselves, except an undefined feeling of straining or fatigue ; sometimes a pressure in that part of the muscles ; a painful drawing of the nerves in the direction of the trunk ; a cold feeling in the whole arm. These morbid sensations only appear as a rule, after strained efforts to overcome the spasmodic hindrance (or impediment) ; probably from their very passing appearance lasting so short a time, are not observed. The whole of the sufferings come after long-sustained and strained employment of the fingers and hands ; and the cramp only appears later. In the case of Valleroud, after a wound from an exploding shell (?), a fragment of which remained for a long time in the wounded middle finger, a constant pain followed, which was accompanied later by a winter's croup, which resisted all attempts at cure. Fanciful peculiar tremblings, as with Romberg's nailsmith, without any other consequence, give rise to severe pains, which only accompany certain movements, and are followed by tonic cramp of the muscles. A smith complained to me from the same cause, besides sensitiveness in the arm, of a frequent firm con-

raction of the fingers of the right hand, accompained by the eeling of its having gone to sleep. These appearances were erceptible in rest, as well as in different movements. I myself nce got severe pain in the muscles of the hand after long-ontinued work at anatomy ; and I was then for a considerable ime subject to it in holding the forceps, and to powerful tonic ontractions of the muscles used for that purpose.

My own experience in the treatment of these cases is very lecided : rest—entire rest from the occupation that has pro-luced the disease. If the disease is recognised early, then wo months entire rest will arrest its progress ; at the end of hat time strychnine is of service, but it is injurious when iven in the early stage of the disease, and before rest. During he time of rest the health should be sustained and improved n every possible way. Fresh bracing air—mountain air if ttainable—is of more service than all the tonics in the world. As regards tonics, they must be varied to suit the peculi-rities of each constitution. Zinc, iron, and quinine are the onics on which I rely most.

The following is one of the most striking cases of paralysis I now of, induced suddenly by little more than an hour's con-inued strain upon a nervous centre, confirming all that I ave said regarding the controlling and co-ordinating power f the spinal cord :—A gentleman got into the five o'clock xpress train for Brighton. He was no sooner seated than e felt a strong desire to have the bowels relieved ; but there vas no help for it—the train started, nor did it stop till it eached its destination at a quarter past six o'clock. All this ime the desire to go to stool increased more and more, but he estrained it successfully by a violent effort. The moment the rain stopped he sprang out of the carriage, when the sphincter ani relaxed, and the contents of the bowels were poured out nto his clothes. The curious part of the case is, that he ecame paraplegic. The spinal cord had been over-exerted, exhausted, and paralysed by the long-continued effort. In his case the brain, from which I suppose all must admit that he will emanated by which he desired to control his sphincter, did not give way. His intellect was not impaired, and the whole muscular system was not paralysed, only that portion which is supplied by the lower portion of the cord. I regard his as identical in its pathology to scriveners' palsy, only that he destruction of the nervous centre was sudden, not gradual. I do not know what treatment was adopted in this case, but, reasoning from analogy, I should have advised complete rest ; hat some arrangement should have been made to prevent his ever having to control the action of the bowels if he had retained in any degree that power.—*Lancet, Dec.* 24, 1864, *p.* 709, *and Feb.* 4, 1865, *p.* 114.

10.—ON MUSCULAR ATROPHY, OR WASTING PALSY.

By Dr. C. B. Radcliffe, Physician to the Westminster Hospital, and to the National Hospital for Paralysis and Epilepsy, &c.

The disease upon which I am about to make a few comments has been distinctly recognised only within the last ten years. It is that disease which is now generally regarded as primary idiopathic granular or fatty degeneration of the muscular fibre, analogous in all essential features to fatty degeneration of the heart; and to which the names of progressive muscular atrophy, wasting palsy, or Cruveilhier's disease, are most generally given —the latter name in honour of the distinguished physician who was the first to call attention prominently to the subject. The case which will serve me as a text terminated in perfect recovery, and this termination is not the smallest feature of interest belonging to it; for the common impression is that the disease of which it is an example is one which sooner or later ends fatally. This is the impression, for example, which is gathered from the perusal of M. Trousseau's excellent clinical lecture on the subject, seeing that not one single word is said in this lecture on remedies or cure. Moreover, the very epithet *progressive* implies, more or less distinctly, a fatal tendency. In fact, however, there are other cases on record which show that a less gloomy impression is justifiable; and that this is so, abundant proof may be found in the excellent work of M. Duchenne (de Boulogne) on "Electrisation Localisée," and in the model essay of Dr. Roberts on "Wasting Palsy."

Case.—Caroline B., aged nineteen, of Ticehurst, Sussex, a domestic servant, admitted into the National Hospital for Paralysis and Epilepsy on the 24th of March, 1864.

Present state.—The patient complains of weakness and wasting of the left hand, and says there is nothing else the matter with her. She also says that this wasting and weakness began five months ago only.

In the left hand, the muscles forming the ball of the thumb (the thenar group) are not more than a third of the size of the corresponding muscles in the other hand, and the hollowness of the metacarpal interspaces is such as to show that the interossei and lumbricales muscles have disappeared in great measure. The special muscles of the little finger (the hypothenar group) are also considerably wasted. The fingers are curved in the position which belongs to incipient "bird's-claw deformity" or "main en griffe"—the deformity which M. Duchenne (de Boulogne) has shown to be the direct result of atrophy of the interossei and lumbricales muscles. The wasted muscles present no signs of fibrillary or other involuntary movement. Those belonging to the ball of the thumb contract freely under

the action of the magneto-electric current; but no sign of contractions can be produced in this way in the spaces where the interossei and lumbricales muscles ought to be. No numbness is complained of at the present time, and none can be detected by the compasses. Comparing the left hand with the right, the surface is found to be cooler, bluer, and moister in the former than in the latter. The pulse at the wrist is also more feeble on the left side than on the right. The muscles of the left arm and of the rest of the body present nothing wrong in form or function: indeed, with the exception of the condition of the left hand which has been described, the patient (who is a short, healthy-looking, well-developed florid girl, with dark hair, light-grey irides, and large but regular pupils) would seem to be in perfect health, both bodily and mental.

Previous history.—Nearly five years ago the patient noticed that her left hand was weak, wasted, and occasionally numb. About the same time the little finger became bent, and got "into the way;" and from that time to this the condition of the hand has gone on gradually altering into that which exists at present. Latterly, she says, the hand may have "gone wrong a little more quickly." She also says that from the first the fingers would frequently "die," particularly when cold, and recover with "pins-and-needles." Of late, also, there appear to have been occasional attacks of palpitation.

Previous to the commencement of the wasting of the left hand, the patient appears to have enjoyed uniformly good health; and she distinctly says that she never sprained or hurt this hand in any way. Her family history is also good. She is one of ten brothers and sisters, all living and well, as she believes, except three, who died of fever; and both her parents are living.

Treatment.—Faradization, kneading, and suitable localized movements to be used every day. Half a fluid drachm of syrup of iodide of iron to be taken thrice a day, with full diet and porter.

Progress of the case.—April 9th. All the treatment carried out. Faradization has been used eleven times. Slight action in the lumbricales and interossei muscles.

20th. Is now able to straighten the first and second fingers, and to do something towards straightening the third and fourth. The hand is warmer to the touch. The Faradization has been repeated eighteen times.

May 13th. The hand has almost entirely recovered its full form and functions. The Faradization has been repeated twenty-six times.

June 23rd. The wasted muscles have now fully recovered their bulk and uses. The form of the left hand is now perfectly

natural, and its bulk quite equal to that of its fellow. The patient, in fact, has perfectly recovered, and is to-day discharged *cured*.

This case is not an example of Cruveilhier's atrophy in its most marked degree. It is not one in which the arms, reduced to little more than skin and bone, hang uselessly by the sides; or in which the chest is so far stripped of muscle that scarcely enough power is left to carry on the breathing; or in which the tongue is wasted to such a degree as to be of little or no use in speech or mastication; or in which the pharyngeal muscles are attacked, and death from starvation can only be averted by using the stomach-pump. But it is one which may serve my present purpose not indifferently.

The atrophy of the muscles is the first point to which the attention had best be directed. The muscles of the left hand, especially those in the metacarpal interspaces, are considerably wasted; while the muscles of the left arm and of the rest of the body are perfectly plump and healthy. Now this wasting is peculiar in more respects than one. Some muscles are, as it were, dissected out, leaving hollows which seem all the more hollow from the fact that the neighbouring muscles may be almost or altogether unchanged. This wasting is partial in a sense which forms a marked contrast to that general wasting which occurs after paralysis. There is also something very peculiar in the situation of the atrophy. At first the impression may be that the wasting of the muscles is the result of some paralysing lesion in some nerve or nerve-centre; but it soon becomes evident that it is difficult to account for the selection of the muscles upon this view. The muscles, in fact, are not selected in those groups in which they are affected in hemiplegia, in paraplegia, or in any other manner which points clearly to some paralysing lesion in a nerve or nerve-centre. There seems to be something capricious in the selection of the muscles which are the seat of disease; unless it may be that the obscure disposition to a symmetrical affection of corresponding muscles on the two sides of the body may point to some disease in the blood, rather than to any disease in the cerebro-spinal nervous centres, or in the nerves belonging to these centres. Moreover, the wasting in the disease in question is an early phenomenon—perhaps the one first mentioned, and not a late one, as it is in the wasting which is consequent upon paralytic affections.

Weakness is the symptom which has next to be noticed. The muscles of the hand are weak as well as wasted. Now, this weakness is quite distinct from paralysis. The muscles waste, and as they waste they become weaker and weaker; but so long as any muscular structure remains, the will is capable of acting upon it, and the degree of muscular force called into

action in this way is proportionate to the amount of unaltered muscular structure remaining. *There is no true paralysis.* The relation of the weakness to the wasting is also distinct, the weakness being posterior to the wasting, not anterior. The case cannot by possibility be confounded with sudden paralysis from cerebral or spinal disorder; nor need it be confounded with that paralysis arising from softening of the brain or spinal cord, which may be ushered in slowly, and in which the muscles may waste *pari passu* with their loss of power; for in this latter case the hemiplegic, paraplegic, or otherwise "regular" character of the paralysis, and the presence of other unequivocal "head symptoms" or "spinal symptoms," will remove all doubt as to the true nature of the malady, if only moderate care be used in the examination.

Numbness of the hand is one of the symptoms mentioned in our case; but this numbness is not a constant symptom. It could not be detected at the time of the first examination, and it was not detected afterwards. It came on when the hand got cold, as the hand very readily did. It seemed to be rather the result of want of blood from failure of the circulation in the part (the circulation in the part was at first very feeble) than anything else: so that this symptom cannot be looked upon as a sign of any disease in the ordinary nerves of the hand, or in the nerve-centres belonging to these nerves. Nor does numbness or pain or any other sign of sensory disturbance occur as a constant feature in other cases to invalidate this conclusion. On the contrary, the remarkable freedom from such disturbance in cases of Cruveilhier's atrophy is one of the most striking features of the disease.—*Lancet, Jan.* 14, 1865, *p.* 30.

11.—EPILEPSY AND THE ADMINISTRATION OF BROMIDE OF POTASSIUM.

By Dr. G. Goddard Rogers, Physician to the West London Hospital, and late Medical Registrar to St. George's Hospital.

[The good results following the administration of the bromides in cases of epilepsy are undoubted. It cannot but result in disappointment if it is expected to be equally beneficial in every case, and our next endeavour must be to ascertain to what class of cases it is most suitable. The following cases are interesting.]

Case 1.—R. B., aged thirty-four, married, with two children, the youngest eleven years old, first came under treatment at the West London Hospital in May, 1861. About six months after marriage she miscarried, and had for three weeks frequent attacks of syncope, followed a few weeks later by a convulsive seizure, during which she lost her consciousness. She does not

remember being told afterwards whether she foamed at the mouth or bit her tongue. These attacks were few in number for some years; but she often had "fainting fits" and "sighing fits," during which she "felt lost"—*(petit mal?)*

In the early part of 1861 the fits recurred more frequently; so that she was obliged to give up her employment as occasional nurse. Feeling low-spirited, and despairing of aiding her family, she came to the hospital. She was rather below the middle height, of florid complexion, with a wild staring expression, and very excitable manner. The heart's action was somewhat feeble, the breathing tranquil, and the bowels apt to be somewhat relaxed. Pressure of the crowd in the waiting-room, or the anxious struggle to be amongst the first served with medicine after seeing the physician, would sometimes bring on a seizure. One I particularly remember was most violent, and lasted a long time. Although the true fits, or, as she always termed them, the "struggling fits," were principally confined to the pre-menstrual or post-menstrual epochs, yet they occasionally followed coition, as I learned from the husband, who, by the way, has been a noted pedestrian in sporting circles during the past twelve years.

I first gave turpentine and castor-oil draughts; but no worms were dislodged. Bichloride of mercury with bark was next tried; and remembering that valerian had been mentioned by some authorities as putting in a claim for approval, I tried it in many forms—infusion, ammoniated tincture, valerianate of iron, of zinc, and of quinine. Dr. Duncan Gibb, who was about that time my colleague, had been giving the bromide of ammonium to allay irritability and decrease sensibility of the larynx and pharynx prior to using the laryngoscope. Thinking the same drug might act beneficially on the mucous lining of the uterine organs, I administered it freely to my patient; but with no good effect. The same was the case with iodide of potassium; and I then accidentally refreshed my memory by reading Sir C. Locock's paper on the bromide.

From the 21st November, 1862, to the 3rd December, R. B., took bromide of potassium in four-grain doses three times a day, with compound tincture of valerian Between these dates she had two fits of a mild character. The medicine was intermitted until the 9th, when she came in great distress, saying that she was suffering from severe flooding, and had had several violent fits, on one occasion nearly falling into the fire. She was ordered to resume the medicine.

Jan. 2nd, 1863. To leave off the tincture of valerian, and take ten grains of the bromide in water three times a day.

8th. Two severe attacks.

16th. No seizure since last entry.

She now took quinine and ammonia up to March 16th. The seizures were very frequent during a portion of this time, and she suffered severely from menorrhagia.

From the 13th March to the 12th May she took ten grains of the bromide three times a day, and much improved. In fact, she relied solely on the medicine, and declared that the fits would return directly she left it off. To test this statement, during the latter half of July I merely prescribed a placebo of aromatic water; but so great was her distress that she implored me to allow her to resume the original medicine, and during August and September she took fifteen grains of the bromide three times a day. I afterwards increased the dose to a scruple. Beyond this dose I never found occasion to go with the patient, and, from my experience in other cases with larger doses, I am compelled to differ from Dr. M'Donnell when he says that ten grains three times a day is "too small a dose to develope any good result."

In January, 1864, I discharged my patient, she not having experienced a severe fit, or one of the *petit mal* description, for more than three months. She resumed her former work of nursing, and I saw nothing more of her until the 10th of June last, when she came to me complaining of vertigo and "fulness about the throat," the "choking aura," which was the sure precursor of a seizure at a menstrual period. Two days later the seizure occurred, but it was of a very mild type. I kept her on scruple doses of the bromide up to the 15th July, seeing her during this time on six occasions. She remained quite free from attacks up to the time of her dismissal in July, and on the 3rd November I heard that her good health had been uninterrupted. I do not mean to say that she is cured; but in 1861 and 1862 her life was a burden in consequence of frequent attacks, and from these she has enjoyed an immunity for three, four, or five months.

Systematic writers on materia medica have dwelt much on the anaphrodisiac influence of bromide of potassium. I do not think its beneficial influence is to be looked for in a simple lowering of the sexual power. When that power is unduly drawn upon, a degree of irritation is produced which the bromide may calm. But in the case above related, I had the fullest opportunities of learning from the husband that there never had been any diminution in his wife's *empressement*. And to arrive at greater certainty, the husband came under my care for some trivial ailment during his wife's attendance at the hospital, I took the opportunity of administering the bromide of potassium to him in fifteen and twenty grain doses three times a day. The information supplied after the lapse of a month satisfied me that here, at all events, the depressing property of the drug was

nil. Where there is irritation and frequent priapism, the result of onanism or venereal excess, and where there is every reason to conjecture that such irritation is at the bottom of the epileptic seizures, then the bromide of potassium is, I think, of service. Dr. M'Donnell has promised us some further observations which it is probable will throw still more light on this important subject.

As confirmatory of the conjecture hazarded above respecting epilepsy in the male subject, I add a few notes of an hospital case which will bring this paper to an end.

Case 2.—E. B., aged sixteen, came to the West London Hospital in June last. His appearance is heavy, he complains of loss of memory, is partially deaf, and the pupils are much dilated. At twelve years of age, when working in a gun-factory, he contracted bad habits, which he has continued to indulge up to the present time. Sometimes he has practised self-abuse three and four times a day. The bowels are costive, and there is a feeling of weight at the vertex. Before he was thirteen years of age he had several fits, which his friends described to me as most severe, his struggles became extremely violent, and his tongue often bitten through. There was an intermission of eighteen months, followed by a fresh series of fits; and he then went to Salisbury Infirmary. The change of air and the medical treatment effected but little good; and when he returned to London he had "twitching of the face," and fits almost every day. In June and July he was under my care, and I gave him the bromide three times a day in doses gradually increased to twenty-five grains. The attacks gradually became less frequent, and he was entirely free from them all through August. On the 2nd of September his friends brought him again, as he had suffered from a slight fit during the night. I resumed the bromide; and, after taking it a fortnight, he felt better, and had no "twitchings" and no sign of a fit. Up to the present date (Nov. 21st) this lad has had no further seizures; and I think it reasonable to regard his improvement as due to the influence of the drug, coupled with obedience to my enjoining a strict abstinence from vicious habits.—*Lancet, Dec.* 10, 1864, *p.* 656.

12.—CASE OF GENERAL HYSTERICAL PARALYSIS TREATED BY THE CONTINUOUS GALVANIC CURRENT.

Under the care of Dr. SAVAGE, at the Samaritan Free Hospital.

An unmarried woman, aged nineteen, was brought to the hospital by her mother in September last, to ask the advice of Dr. Savage. She had always been in indifferent health, and during the last three years gradually lost the power over her

arms and legs to such an extent that she is scarcely able to walk even when supported, and entirely incapacitated from doing any work whatever. She first menstruated at fifteen years of age, but has always been very irregular, and the discharge very pale and slight. Sixteen months ago the catamenia ceased altogether, and from that time she became nearly idiotic. At present she is very listless, has a vacant look, and considerable dilatation of both pupils, more especially of the left one. The left iris is scarcely at all influenced even by a strong light. Her voice is almost entirely gone: she can only speak in a faint whisper. She is frequently troubled by pain in the head and the back. Her hands and feet are always quite cold. She complains of sickness in the morning, total want of appetite, and constipated bowels. She has had hysterical (epileptiform) fits. She has been under medical treatment for a long time, but without any benefit.

Seeing this condition of the patient, Dr. Savage consulted with Dr. Althaus, with the view of applying some form of galvanic electricity in order to rouse her nervous system. The latter gentleman made a most careful examination of the state of sensibility all over the body, and found that there was nearly complete anæsthesia of the whole left side, including the conjunctiva. On the right side the loss of sensation was not so much marked as on the left; but the prick of a pin was only felt as if it were a touch by some blunt instrument, and the examination by Weber's compass-æsthesiometer did not lead to any results, as the patient could nowhere distinguish whether she was touched by one point or two. The muscles appeared to be very badly nourished, but they contracted tolerably under the influence of the electro-magnetic current.

In cases like the foregoing, the application of the continuous galvanic current, such as produced by a large number of cells of Bunsen's or Daniell's battery, feebly charged, generally proves curative. It is a matter of doubt whether the current acts directly on the spinal cord, as is asserted by Prof. Remak, or whether it merely acts on the nervous centres by reflex action from the sentient nerves of the skin. The latter opinion is held by Dr. Althaus, who thinks that the resistance offered to the passage of the current by the bones and membranes enveloping the nervous centres is too great to be overcome by a current of such tension as it is safe to apply in the human subject. The principle of reflex action, moreover, is quite sufficient to explain the physiological and therapeutical action of the continuous current, so that it is not necessary to go out of our way and assume principles of electric conduction for the human body different from those which obtain for other physical bodies. However this may be, thus much is certain that

the continuous current, if applied in a proper manner, has a distinct action on the nervous centres, such as is not possessed by the interrupted current. In cases, therefore, where the affection proceeds from a lesion of the nervous centres, the continuous current may be used; while for local ailments invading peripheral nerves, the interrupted current is more beneficial.

In the case just mentioned, a current of from forty to fifty cells of Daniell's battery was sent in an inverse direction to the spine, from the nape of the neck downwards to the sacrum, for ten minutes. The operation was repeated twice a week. The pain caused by this proceeding is very insignificant in the majority of cases, and to some patients the sensation is positively pleasant. After six weeks' treatment in the way described, the patient was so much improved that she could walk three miles at a time without support; she could dress and feed herself; was able to do heavy work about the house; had always warm hands and feet; the voice had returned; the bowels acted regularly; and she was cheerful, and took an interest in everything relating to her affairs. The pupils now were quite normal. An examination by means of the æsthesiometer showed that sensation had been quite re-established, on the left as well as the right side. She was discharged on Nov. 19th, being then in perfect health, with the exception of the amenorrhœa, which still persisted, and an occasional feeling of sickness in the morning.

In this case the disorder was evidently of a merely functional character; but cases which are at present under Dr. Althaus's care seem to show that even in severe structural lesions of the cord and its appendages, the continuous galvanic current, if properly applied, produces highly satisfactory results. One of the cases is that of a girl, aged thirteen, who six years ago had meningitis spinalis lumbaris, contracted by sitting in winter on a cold door-step; and who, having become completely paralysed in the lower extremities, was admitted into Guy's Hospital, under the care of Dr. Gull. She remained in the hospital nine months, but without in the slightest degree recovering the use of her limbs. In October last she was sent to Dr. Althaus by Dr. Leared, under whose care she had been for some time previously. When she was first brought under the influence of the continuous current the paraplegia was complete, the patient being perfectly helpless, and the legs being wasted to the last degree. She is now so far improved that she can move her legs in all directions; and although she is not yet able to stand without support, Dr. Althaus holds out the expectation that by continuing the treatment she will entirely recover. The bulk of the legs has considerably increased although no local application of the current to the legs has been made.

The continuous current also proves beneficial in what has been recently described as "ataxie locomotrice" by Duchenne, but which is nothing else than the disease long known to the profession as atrophy of the posterior columns of the cord, and described by Romberg as "tabes dorsalis."—*Lancet, Feb.* 18, 1865, *p.* 175.

13.—ON THE TREATMENT OF CHOREA.

By Dr. James Turnbull, Physician to the Liverpool Infirmary.

[Chorea occurs in females most frequently just before the catamenial function is established, and when it comes on after this time there is generally some menstrual irregularity, accompanied with more or less anæmia. And the connexion between chorea and rheumatism is a most singular and interesting fact, suggesting the inquiry whether they are not both dependant upon the same *materies morbi* in the blood. Dr. Turnbull then passes on to the treatment of chorea, and observes :—]

The following case is interesting, both as an illustration of what may be termed hysterical chorea, and as affording an example of successful treatment by sulphate of aniline, whether such result may be considered to have been due to its direct sedative influence on the nervous system, or partly to the mental impression it produced on the patient—an influence in the treatment of such cases which should not be overlooked.

A lady, aged about 25, was placed under my care on November 16th, 1861, on account of a peculiar nervous affection, from which she had suffered more or less for eight years. In the early period of her illness, she had been under the care of the late Sir B. Brodie, and had afterwards been under the treatment of several irregular practitioners, but without receiving permanent benefit. The nervous affection consisted of an inability to control the muscles of the right arm, and in a slighter degree those of the left. There was an almost constant contraction of the muscles of the right shoulder and arm, causing a painful fidgeting motion, and compelling her to hold the arm with the left hand. There was plenty of muscular power; but she could not regulate the movements so as to write or do any kind of work. In walking, she had not perfect control of her movements, and exhibited some of the peculiar jerking seen in St. Vitus's dance. As regarded the general symptoms, the complexion was rather anæmic; the muscular development good; the tongue natural; the digestion pretty good; the bowels sluggish; the pulse quick; and menstruation regular, but scanty. The treatment was first directed to the improvement of the general health by the preparations of iron

combined with ammonia, and by out-door exercise, the use of dumb-bells, &c. Warm hip-baths, with mustard, were given; sedative embrocations, with atropia, &c., were used to the arm and shoulder; aloetic aperients were administered. By these means, the general health was improved; but no influence was produced on the nervous disorder.

On the 10th of January, after she had been nearly two months under the previous treatment, I prescribed sulphate of aniline in three-grain doses thrice a day. It produced blueness of the lips and face more rapidly than in any other case I have seen; and in other respects, she seemed peculiarly sensitive to the influence of the remedy, which always produced in the evening, after the third dose, not only decided blueness, but also nervous depression, discomfort at the epigastrium, slight headache and palpitation, quickness of pulse, and coolness of the hands. At the same time, it had a marked beneficial influence on the disease; the muscular irritability was rapidly diminished, and she regained the control of the right hand and arm. The depressing effect of the remedy made it necessary to watch carefully its action, and to reduce the quantity to two doses daily. It was continued for three weeks, with occasional intermission for a day; and she had then completely regained the control of all her muscles, and could play on the piano. The blueness in this, as in all cases, went off within twenty-four hours after laying aside the remedy. About the middle of February, she had some return of the nervous symptoms, which soon yielded to the same treatmemt; and, at the end of the month, she returned home quite well.

Aniline has a powerfnl influence on the nervous system when taken internally, and still more when inhaled. From being accidentally inhaled, it has in several instances produced serious poisonous effects, though it has not been known to cause fatal effects on man. In recommending it as a remedy in chorea, I advised that the sulphate should be given in doses of two or three grains; and that it should be diminished or omitted when it produced the depressing effect, which is usually experienced in proportion to the temporary blueness of the surface it causes.

Dr. Frazer, who published some cases in which he gave it unsuccessfully, appears to have administered it in a different way—in large and increasing doses, as sulphate of zinc has sometimes been given—and as if his object had been to test its toxic as much as its therapeutic effects. In one of his cases, he tells us that the patient took twenty-eight grains daily. In thirty-four days, the quantity taken was four hundred and six grains; and it was continued all this time, although it caused frontal pain and aggravated deafness. "The result," he observes, "points nevertheless to the efficacy of sulphate of aniline in five

cases; and is, therefore, *pro tanto*, a clinical fact to be noted." I believe that any agent which produces a violent impression on the nerveus system may prove injurious in this disease; and I advise the employment of aniline only so as to produce a gentle sedative influence; and, as not one out of five cases thus treated in hospital improved or recovered under this mode of treatment from a disease which Dr. Wilks has shown that many patients get well of under regimen only, the result would seem to show further, that thus given, as an antidote against the disease, it may prove not merely inefficacious, but injurious.

Having referred to the toxic properties of aniline, I would here observe that they have been erroneously regarded as of similar character with those of a more highly narcotic agent—nitro-benzine—which has very noxious properties, but has nevertheless been much used for flavouring and in perfuming, on account of its resemblance to the oil of bitter almonds. The effects of aniline and nitro-benzine have been confounded together from these circumstances. When aniline is introduced into the system, it undergoes oxidation, by which the mauve colouring matter is produced, as I was the first to point out. When nitro-benzine is introduced either through the stomach or by inhalation of the vapour, a portion is first converted into aniline, and this again undergoes further change, so that the same blueness is caused by both. The effects of nitro-benzine are, however, essentially different, and more persistently narcotic than those of aniline, as has been shown by Dr. Taylor in Guy's Hospital Reports, in a recent paper in which he has fully examined the various cases in which the toxic effects of these agents have been known to be produced; and he has recognised the difference in the effects of aniline and nitro-benzine. There is one fact, however, which appears to have escaped attention. It is this: that nitro-benzine is one of a series of chemical compounds which have similar effects on the nervous system. They are the xyloids—a series of substitution compounds formed by the action of nitric acid on starch (xyloidine), on sugar (saccharoine), on cotton (pyroxiline or gun-cotton), on glycerine (glonoine or nitro-glycerine), and on benzine (benzoine or nitro-benzine. Organic compounds containing nitrogen have often a powerful influence on the animal economy, as is the case with the vegetable alkaloids; and these are all nitrogenous compounds of similar chemical constitution.

Dr. Edwards, in a paper on the Physiological Properties of Xyloids, published in the 'Liverpool Medico-Chirurgical Journal,' first drew attention to the action of these agents, and showed that they have similar and peculiar effects on the nervous system: the amount of their action being, in a measure, proportioned to their solubility. "They all appear" (he says)

"to possess, more or less, the peculiar action on the nervous system characteristic of strychnine; and I believe that further experiments on men would prove that they possess peculiar and valuable medicinal properties. Physically, they are allied; being all highly explosive bodies, deflagrating at a low temperature or by concussion; and physiologically, they all produce a powerful effect on the heart's action, trismus, and a series of tetanic convulsions, terminating in death."

Having witnessed most of Dr. Edwards' experiments I have taken this opportunity, of not only pointing out that nitro-benzine is different in its effects from aniline, but that it is one of the xyloids, and that to Dr. Edwards the merit is due of first ascertaining the poisonous properties of nitro-benzine, as well as its relation to the other xyloids.

Looking at chorea in a strictly practical point of view I would now make a few remarks on the general treatment of the disease. In cancer, which is one of the most incurable diseases, many remedies have been, and will no doubt continue to be, tried; and in chorea, which is one of the most curable, many remedies have likewise been tried. Mere number points, in reference to the first, to its intractable nature; and, in regard to the second, to the fact that, though, generally a curable disease, none of the remedies in use can be relied on to control the disease with any certainity. Indeed, practically, we know that there are cases which for months, and even years, resist both judicious and expectant treatment; and, therefore, the study of therapeutics in reference to this disease is still worthy of attention.

The remedies which have been chiefly used are, purgatives; the mineral tonics—iron, zinc, and arsenic; cod-liver oil; electricity; and the shower-bath. Purgatives I have given only when the state of the bowels rendered their use necessary; and not, therefore, with the curative intention with which they have sometimes been administered. The mineral tonics undoubtedly exert a beneficial influence. I do not believe they have any direct curative power; but they improve the general health, which is more or less below par, and thus they appear to lessen the excitability of the nervous system. Iron I believe to be the most generally useful, and especially in anæmic females. Dr. Hughes, in his digest of one hundred cases, makes these remarks:

"The different mineral tonics are very variously estimated by different practitioners, and one is often regarded as especially suited to the cure of chorea. While one practitioner regards arsenic as a specific for true chorea, another cures nearly all his cases with the sulphate or the oxide of zinc, and another considers the sulphate or the carbonate of iron as an almost infallible remedy. This may perhaps arise, in a great measure, from

habit. It is possible that all may be nearly equally efficacious. I have seen each of these, and many other remedies cure the complaint; and I have seen them all occasionally fail."

Zinc has been given in large doses, especially at Guy's Hospital; where Dr. Hughes has stated that thirty-six grains of the sulphate have been administered daily. It appears such an abuse of medicine, to prescribe these enormous doses of an irritant drug commonly used in such quantities as an emetic, that we are at a loss to discover how it should ever have been thus given. What Sir John Forbes has termed the autocracy of nature, could not have been understood by those prescribing it in such a manner; and Dr. Hughes observes:

"In the practice of the hospital, I am not aware that it has been shown that small or moderate doses of the mineral, continued an equally long time, will not effect a cure."

In my own practice, I have used iron and arsenic more frequently than zinc; and I consider that, of the metallic tonics, iron is the best, and that arsenic stands next to it.

Cod-liver oil is a remedy which is sometimes of service, by improving the general health; and it may be given with, or subsequent to, the preparations of iron. Electricity seems, in some cases, to have acted beneficially, and in a very rapid manner.

The shower-bath is a remedial agent which gives tone to the system, and may be used along with other tonic treatment. I have seen it of the greatest use in many cases. In the case of a young lady, in whom the movements were incessant and violent, and prevented sleep, the movements subsided very much directly after the bath, and she soon fell asleep, and ultimately recovered. The emaciation and exhaustion of the patient from the incessant violent movements and the loss of sleep, had rendered the recovery of this patient very doubtful.

It is not less important to glance now at the means which may prove detrimental, and which should therefore be *avoided* in the treatment of this nervous affection. All violent impressions on the nervous system should be guarded against. We know that fright is not an unfrequent cause of the disease. It is probably on this account that the shower-bath may prove injurious when injudiciously employed, as well as very frequently useful. Narcotic remedies, such as opium, morphia, and hyoscyamus, have appeared to me (contrary to what might be expected) to be not simply useless, but positively hurtful. It has been said that chloroform is of use; but I saw the inhalation tried in two very severe cases, which proved fatal; and though it produced a temporary sedative influence, it appeared to me to act injuriously on the general progress of the disease.

In all cases, due attention should be paid to hygienic as well

as medicinal treatment, so as to improve in every way practicable the nutritive functions and the general health. The diet should be nutritious, and wine or malt liquors are often of service; and out-door exercise is desirable in the less severe cases. When the disease proves chronic, change of air is advisable; and in the summer the change may be made with advantage to the sea-side, where sea-bathing may be practised.—*British Medical Journal, Jan.* 14, 1865, *p.* 33.

DISEASES OF THE ORGANS OF RESPIRATION.

14.—ON INFLAMMATION AS AN EXCITING CAUSE OF TUBERCULOSIS.

By Dr. James Jones, Physician to the Metropolitan Free Hospital, and to the Infirmary for Consumption and Diseases of the Chest.

[There is a very marked analogy between the various cacoplastic formations found in cachectic subjects, and the plasma exuded in ordinary sthenic inflammation, the chief difference consisting in the amount of their vital capacity or power of organisation. In some, the resemblance amounts even to identity. Who, for instance, could draw a line of demarcation between tubercular infiltration and the low plasma found in the pneumonia of debilitated subjects.]

If we commence at the bottom of the series by examining what is known as lardaceous exudation, we find a plasma, if, indeed, it merit that name, which is capable of advancing little or nothing in organization. Its vitality is so low, that as it exudes so it remains, with scarcely a trace of organization.

Advancing a little higher, we find the plasma known as tubercular infiltration. This, although but little superior to the lardaceous, asserts its claim to a higher place amongst the plasmata, by exhibiting a certain amount of capacity for organization in a scanty fibrillation and cellulation.

Next in the ascending scale comes the miliary, or grey semi-transparent tubercle, showing under the microscope a still greater development of fibre and cell.

Then comes the yellow, or common form of tubercle, to which, I suppose, we may give the next place in our ascending scale, inasmuch as it is found to consist of abortive cells, granules, &c., which indicates the presence of a degree of vital power sufficient to originate a cell-growth, but insufficient for its completion.

The next higher place may be assigned to cancer, inasmuch as it exhibits a greater degree of vital capacity for organization;

for, instead of an abortive, we find a well-matured cell-growth. And although incapable of any higher organism, its cells are endowed with the power of reproduction, and consequently of unlimited increase; hence its malignancy.

Next we have a plasma capable of an organization, nearly approaching normal growth, as found in fibrous tumours, &c.

The next point worthy of notice in our comparative examination of the cacoplasmata is their relation to the state of health under which they are formed. We find that their formation is invariably preceded and accompanied by a condition of general cachexia; but we also find that this condition of cachexia may exist for an indefinite time without being attended or followed by the production of any one of those substances. Therefore the condition of cachexia is not, *per se*, capable of giving origin to any one of them: it must be aided by some other factor.

Thus, a person who inherits a cachectic constitution from cancerous parents may live to a good old age, and finally die of natural decay: but a brother or sister may, from the irritation of a tobacco-pipe, or a blow on the breast, die of cancer of the lip or breast.

Again, in a large family, the offspring of consumptive parents, we often find the well-known characteristics of phthisical cachexia stamped on each one; yet some of them will survive to a good old age without a trace of tubercle, while the others die in early or middle life of consumption. Now, it is evident that in the cases of the latter, some new and additional influence came into operation, taking the position of a complementary factor in the production of tubercle (cachexia $+ \times =$ tubercle).

The occasional occurrence of cases in which tubercle is formed in the offspring of consumptive parents without any *apparent* exciting cause, is considered by many well-known pathologists to be sufficient evidence to prove the doctrine of *spontaneous origin;* but it appears to me that such inference is not legitimate, and that it is much more probable that the immediate exciting cause or local factor escaped notice than that it had no existence. There is no caprice in Nature; there must be cause for effect.

We see tubercular cachexia continuing for years, or even a long life, without a particle of tubercle being formed, even under circumstances not the most favourable to the patient's escape. Now, it appears to me that one single instance of this kind is sufficient to upset the theory of the spontaneous origin of tubercle, for it shows that there is something more than a morbid constitutional diathesis necessary in order to bring tuberculosis into action.

Again, in all forms of cachexia, we find the general nutrition

of the body to proceed *normally*, although languidly. The various parts and organs receive a supply of nourishment sufficient for the purposes of life, *but a period at length arrives in which, in some part or organ, an abnormal plasma or blastema is formed*, and this act of abnormal nutrition initiates the local disease. Now, I would ask, What is the cause of this sudden alteration in the nutrition of a part? Is it not the effect of some new power coming into operation, local in its action, and capable by its presence of perverting nutrition? What is this new power or factor?

I will now attempt to show by typical examples that in a large number, if not in all, of at least one form of cacoplasm, namely, tubercle, inflammation is the active factor or additional power sought for, not the sthenic inflammation of vigorous subjects, but inflammation of a low type, a type modified by the constitutional diathesis, whose products are low, imperfectly vitalised, and incapable of assuming any high degree of organization. (In the term "inflammation" I comprehend all those abnormal conditions of the circulation and innervation of a part which give rise to effusion of some form of coagulable lymph or plasma, or to an abnormal increase in functional activity of organs under their influence. I include in it, therefore, the condition commonly called "irritation" as one of its phases.)

I shall also point out the importance of watching for the earliest manifestations of inflammatory action, and the early use of such remedies as science or empirical experience indicate for its removal, and thus, by anticipation, to oppose the onset of tuberculosis; also the necessity of contemporaneously pursuing a constitutional treatment suited to the peculiar nature of the cachexia.

Those who have an opportunity of seeing large numbers of cases of consumption must be struck with the fact that a majority of patients attribute the origin of their illness to "catching cold," or an "attack of bronchitis," or "inflammation of the chest," and that, if careful inquiy be made, their statements are found to be correct.

The following case is a type of a numerous class in example :—A patient, although showing symptoms of low health, is to all appearance free from actual disease until, through some indiscretion, a cold is caught. Cough comes on, with tightness of the chest, and chills, alternating with flushes of heat, are felt. After, a white expectoration appears, the cough continues, the urine is loaded with lithates, the tongue is coated, and the appetite fails; auscultation reveals nothing more than the ordinary signs of bronchitis, and *there is no dulness on percussion.* The patient hopes soon to get better of his cold, but he is dis-

appointed; the cough continues, the expectoration is no less, and is not unfrequently tinged with blood. He loses flesh and strength, and is easily made to perspire. By-and-by, if the chest be examined, *dulness* under or behind one or both clavicles, or the supra-spinal ridge of the scapula, *is readily made out.* Tubercle has formed. This is not an ideal case, but one of a very large number, the history of one of which is the history of all. It is pulmonary phthisis, originating in catarrh. The patient was in pretty good health, or, at all events, free from tubercle until the continued disturbing influence of the catarrhal inflammation produced a condition of active tuberculosis.

I will next give a case of acute tuberculosis, consequent on severe bronchitis. It is taken from the work "On Diseases of the Chest" by Dr. William Stokes, p. 392:—"A patient previously in good health is suddenly attacked with the most violent symptoms. There is high fever, extreme dyspnœa, lividity, and tenacious expectoration. *In the early periods the chest sounds clear*, but the signs of the most intense bronchitis, affecting the tubes of all diameters, are universally audible. The symptoms and signs continue with unabated violence, and *after some time the whole chest presents a certain degree of dulness.* The patient dies from the violence of the pulmonary inflammation, and on dissection every bronchial tube is found inflamed, and the lung equably and closely studded with miliary and granular tubercles."

I have selected this highly acute case from the pen of an eminent pathologist and careful observer, as showing most clearly, although casually, the fact that the dulness on percussion is not found until the inflammation has existed for some time, thus establishing the chronological sequence of the tuberculosis to the disturbing action of the inflammation. In the more common forms of miliary tuberculosis, the symptoms of antecedent bronchitis are less strongly marked, but to the careful inquirer they are unmistakably evident. The cooing, or musical râle, and the moist râles can always be found on full inspiration, but the *diminution in resonance on percussion* begins to manifest itself only *after bronchitis has existed for some time.* We have now the same sequence of signs, the chest resonant in the early part of the attack becoming dull to the stroke after the inflammation has continued for some time. Now, if any faith may be placed in physical diagnosis, we have in this sequence of signs sufficient to prove the fallacy of the doctrine, that irritation arising from the presence of tubercle produces the bronchitis.

Again, if we watch closely the progress of ordinary phthisis, we find each fresh attack of tuberculosis to be preceded by the usual signs of bronchitis. A. D——, æt. twenty-two, came

under my care at the infirmary for Consumption. On examination, I found dulness on and under the cleft clavicle and on the suprascapular ridge, with feeble respiration and prolonged expiration in the same parts. She one day got a severe wetting and took cold. On examination, there was *no alteration* in the extent of the *dulness on percussion*, but auscultation revealed a new set of signs, moist and cooing notes in the apex of the opposite lung, and also in the lung first affected, but extending far below the seat of the percussion dulness. No further change of signs or symptoms occurred until three weeks had elapsed, when dulness began to be detected in the apex of the right lung, and was also found to have extended lower than before in the left lung. In fact, the inflammation had given rise to a fresh crop of tubercle below the original deposit in the left lung, and a primary crop in the apex of the right. Thus, we find the same chronological sequence of tuberculosis to inflammation in the progress as in the origin of tubercle.

This sequence of signs is so constant, that we may predict the presence of tubercle above by a lurking bronchitis below, which is absent in the corresponding portion of the opposite lung. (Stokes.)

I will now examine ordinary chronic bronchitis as a cause of tubercle:—C. B., when to all appearance in good health, caught cold at the beginning of the winter; cough and spitting, with more or less dyspnœa, continued to torment him all through the winter, but passed off when the warm weather set in. The next winter the same symptoms returned, and again passed off in the summer. My notes say, "Except for the cough and dyspnœa the patient appears in good health, the digestive organs are in good condition, and there is no loss of flesh or strength. *The chest sounds well on percussion.* On the return of the fourth summer, the patient expected the cough to subside as usual, but it continued to trouble him all through the summer. On percussing chest, *some dulness was now found* to exist in the *apex of one lung*, and after a lapse of some weeks *the dulness was found to have increased*, and to engage the *apex of the other lung also.* He now lost flesh and strength, but not rapidly. His case proceeded slowly to a fatal termination. Now, in this case, so long as it remained an ordinary chronic bronchitis, the patient maintained flesh and strength, but the advent of dulness on percussion was contemporaneous with the loss of both, and clearly fixes the time at which tuberculosis set in. I have notes of a large number of cases of this kind, which clearly exemplify the same chronological sequence of dulness on percussion to the disturbing influence of chronic bronchitic inflammation.

These cases require careful and frequent examination in order to guard against error of diagnosis. "Dilated bronchi will some-

times so closely mimic advanced phthisis, that a post-mortem inspection can alone remove our uncertainty; but the progessing dulness on percussion, the regular progess of the symptoms, and the absence of the bronchial breathing and resonance of the bronchial voice, which exist in dilated bronchial tubes," are generally sufficient to enable us to differentiate between them.

I will now give a case or two of tuberculosis from parenchymatous inflammation of the lung. Jane S., aged twenty, four years ago, when apparently in very good health, was seized with inflammation of the left lung, for which she was treated by an eminent physician to whom her father was coachman. She kept her bed for six weeks, and has never since recovered her strength nor felt as she did before. About one year ago she had a sharp attack of hæmoptysis. On examination, the right lung was found to be resonant throughout, and the respiration highly puerile. The left lung was dull throughout, the dulness being most intense in the apex and diminishing downwards to the base, but there was no ægophony. In the infra-clavicular region there was well-pronounced gurgling, posteriorly and inferiorly coarse moist râles, cough very troublesome, some nummular sputa, loses flesh, sweats at night. The father of this patient is suffering from some chest affection, supposed to be phthisis. This case is a type of a large class, in which a constitutional tubercular diathesis is roused into a condition of active tuberculosis by local parenchymatous inflammation.

In the following case inflammation and debility, induced by the mode in which it was treated, appear to be the sole factors of tuberculosis:—George E., admitted under my care at the Metropolitan Free Hospital. Tall, thin, sallow complexion, face marked with small-pox. Chest flat, left side somewhat collapsed, nails curved; mother living, and healthy; father killed by accident. Was in perfect health up to three years ago, when, being then a soldier stationed at Shorncliffe, he was "told off" with a party of men to bathe in the sea, at the end of October. Chills came on the same evening, followed by fever and pain in the left side, with dyspnœa and expectoration of red-stained glutinous sputa. He was bled largely from a vein in the arm, and cupped four times. This was followed by extreme weakness. He remained in the Military Hospital thirty-two days, and was then discharged as unfit for service. He has never since regained health and strength. About two years ago hæmoptysis came on, and it has recurred from time to time ever since. The day prior to his admission it was very copious. He expectorates half a pint of thick yellow phlegm every day, and loses flesh rapidly. The left pleura now contains no fluid, but there are well-pronounced signs of a large cavity in the superior part of

the left lung. There are also signs of condensation in the apex of the right. Had this poor fellow been treated less *heroically*, he would probably have recovered from the effect of the inflammation; but the vital powers being reduced by the treatment, the plasma exuded was probably of a very low type, which, if not tubercular *ab initio*, rapidly degenerated into tubercle.

I will now relate a case in which tuberculosis was excited by an attack of pneumonia, in a subject debilitated by the action of other disease. Charles D., a sailor, was attacked by fever on the West Coast of Africa. During convalescence, between two and three weeks before his arrival in the port of London, he caught cold from exposure to the weather. Chills and flushes, cough, and dyspnœa came on. On his admission to the Metropolitan Free Hospital, the right lung was found to be somewhat dull on percussion in its lower and posterior part, and auscultation revealed a fine crepitation. The patient had lost flesh, and was very weak. He continued in this state, without any improvement, for some weeks. There was occasional hæmoptysis. The apex of the same lung then became dull on percussion, with coarse moist sounds. The expectoration was a copious muco-pus. He became extremely attenuated, had night sweats, and frequently a sharp diarrhœa. He was removed into the country by his friends, and I lost all trace of him, although he promised to write.

One more case, and I have done. J. B., aged thirty-five, of irregular habits, obtaining a living by "jobbing" about the docks and river, had been out of work and badly fed, when, on going to a "job," he fell into the water and then allowed his clothes to dry on him. The next night he had shivering and flushes of heat. On admission to the Metropolitan Free Hospital, crepitation, rather fine, was audible in the posterior inferior part of the right lung. His symptoms were not severe, but remained unchanged for a *month*, and then slight dulness began to appear. The crepitation became coarser, and in three months from the commencement of his illness the signs of a cavity were obvious. The expectoration became copious and nummular. He lost flesh rapidly, and died with all the symptoms of pulmonary consumption.

We can differentiate between these cases and mere pneumonia, by the long continuance of the crepitation, by the absence of bronchial or tubular breathing, by the slight degree of dulness, and the lateness of the period at which it manifests itself. Also between them and capillary bronchitis by one lung only being engaged (a rare thing in bronchitis), and by the slow progress of the symptoms as well as by the general history of the case.

I will merely allude to the inhalation of stone-grit, &c., as a cause of tuberculosis (a subject already ably handled by Dr.

Peacock), although its bearing on the question under discussion is most important, inasmuch as by it we can trace tubercle back to inflammatory irritation caused by the presence of a mechanical irritant in previously healthy subjects. In all those cases *dulness is a late symptom*, appearing only after the health has been impaired by the continued bronchitis. I have seen cases of this form of tuberculosis in granary men, who inhale the silicious dust from the tunic of the grain while shovelling it. The grinders' rot of Sheffield and Birmingham is a form of tuberculosis produced in a similar manner by inhaling particles of steel cast off in the process of dry-grinding cutlery, &c.

The above cases are sufficient to exemplify the important position which inflammation holds as an exciting cause of tuberculosis; but we must, at the same time, avoid the grave error of ignoring a predisposing cause.

Inflammation does not, I believe, in any case give rise to the formation of tubercle in a healthy subject. There must be also a condition of low health or cachexia present. This cachexia may be either inherited or acquired. In some of the cases I have related it was of the latter character. In the soldier it was probably caused by the depletion, in the sailor it arose from the fever, in the dockyard man from irregular living and insufficient food. I would argue, therefore, that neither the cachexia nor the local inflammation is capable *per se* of producing tubercle, but that tuberculosis is the result of their combined action.

It may be objected by some that the inflammation in these cases was the consequence of irritation produced by tubercle already present, but the difficulties attending this theory are much greater than the other; for, in the first place, the physical signs of tubercle are at first wanting, and show themselves only after the inflammation has continued for some time, and, in the next place, there is the difficulty of comprehending how a few soft or semi-fluid tubercles in a yielding and easily-compressed structure like the lungs could possess such irritative qualities, or why they should, after remaining dormant for an indefinite time, suddenly assume the character of irritants capable of exciting inflammation. Viewing tubercle as a cacoplasm, it must be subject to the same general laws as all other plastic exudations. Consequently, it must be the result of a disturbing action. As well might we attribute pneumonia to the presence of the plasma which we all admit to be a product of preceding inflammation. If the presence of tubercle in the lungs were capable of causing bronchitis or pneumonia, why are those consequences not always found? Why is the caprice? Why is the effect not constant?

I shall now give a brief recapitulation of the points exemplified in the foregoing cases.

1st. That a condition of low health or cachexia, although a necessary factor in the production of tubercle, may exist for an indefinite time without giving rise to any local lesion.

2nd. That, for the production of tubercle, some additional factor, in the form of a local exciting cause, is necessary.

3rd. That inflammation, in the form which it assumes in cachectic subjects, is capable of acting this part, and is probably the only exciting cause of tuberculosis.

4th. That a close analogy exists between the cacoplastic or lowly-vitalized formation found in cachectic subjects and the more highly-vitalized products of sthenic inflammation in healthy persons, and that this analogy is especially close in the substance called tubercle.

5th. That the condition of constitution known as tubercular cachexia, from whatever cause it may arise, whether an inherited vice of constitution, or the result of depressing agents, is capable of modifying the products of inflammation so as to give them the characters of tubercle.

6th. That the varieties of tubercle depend on the acuteness or chronicity of the inflammation, the degree of the cachexia, and the nature of the structure on or in which they are formed.

7th. That the presence of tubercle in its first stage is insufficient for the production of inflammation.

8th. That the doctrine of spontaneous generation of tubercle is fraught with danger to the patient by diverting the attention from watching for and guarding against the immediate exciting cause, and is also calculated to make our treatment one-sided by being directed solely to the constitutional malady.

9th. That, in the treatment, our remedies must be directed both against the constitutional vice and the local malady, and that both indications may be fulfilled contemporaneously.

When the subject of tubercular diathesis is seized with catarrh, bronchitis, or other pulmonary inflammation, or with pain in the clavicular or humeral region, with fever and sonorous or moist râles in the corresponding part of the lung, his state is highly critical and entails a fearful amount of responsibility on the physician who undertakes the treatment; for if the local inflammation or irritation be allowed to proceed, tubercle will form; but if, in his zeal for its removal, he adopt measures productive of permanent debility, such as leeching, cupping, purging, and very low diet, he aids powerfully the constitutional vice, and lessens, if he do not destroy, the patient's chance of recovery. If, on the other hand, he looks only to the constitutional malady, leaving the local mischief to the *vis medicatrix naturæ* and doses his patient with beef-steak and brandy, &c., a crop of tubercle is the sure result. The most successful practitioner is he who, keeping in mind the ever-

present cachexia, deals with the local inflammation as the immediate cause of tubercle. The local inflammation being of a low type and slow in progress, it is easy to combine the treatment necessary for both indications. I have found excellent results from a combination of ammonia salines with tartarised antimony and morphia. When the inflammatory action is severe, this treatment should be continued for some days, aided by blisters of acetum cantharidis or other counter-stimulants over the affected parts, while the strength is supported by beef-tea and other light nutriments. The tartarised antimony appears to control inflammation without producing any permanent debility. As soon as the inflammation is somewhat subdued, a combination of perchloride of iron and nitrate of potass, given contemporaneously with the antimonial saline, is eminently useful. When the inflammation is less severe, the two modes may be combined from the very commencement. The dose of tartarised antimony should be much smaller than is usually given in sthenic inflammation. I find from ten to fifteen minims of the antimonial wine of the London Pharmacopœia every four hours to be sufficient. I know of no drug which possesses so powerful a sedative action in pulmonary inflammation as tartarised antimony. In small doses it calms irritation, while at the same time it promotes secretion. Coupled with morphia or cannabis Indica, and given with ammonia salines, it rarely fails as a remedy for low pulmonary inflammation.

The topical application of hot steam to the bronchial mucous membranes by inhalation is a valuable adjunct to the other treatment. When the inflammation is dry, not attended with secretion of mucus, the vapour of hot water alone is very useful. It should be inhaled at frequent intervals, and at as high a temperature as the patient can comfortably bear. Its effects are so soothing that few patients fail to repeat its use. When the sputum is viscid and hard to expectorate, vinegar may be added to the hot water. The French white wine vinegar, owing to the presence of a peculiar aroma, is usually most grateful to the patient. It can be got at Crosse and Blackwell's, Soho Square. When the cough is troublesome, the addition of acetum opii (Pharm. Dubl.) is useful, as are also chloroform or spirit of chloroform, Hoffman's anodyne ether, and Battley's sedative solution of opium when inhaled conjointly with hot water vapour. When a more perfect inhaler is not at hand, the ordinary small circular stoneware teapot is a very good substitute. The vapour is inhaled through the spout, removing it for expiration and depressing the handle to keep the liquid contents from entering the spout.—*Medical Mirror, April* 1865, *p.* 202.

15.—ON FEVER AND BRONCHITIS AS EARLY SIGNS OF PHTHISIS.

By Dr. James Russell, Physician to the Birmingham General Hospital.

[The most common form of pulmonary inflammation which attends the deposit of tubercle is bronchitis. The bronchitis of early phthisis possesses two especial characteristics. It is so mild that under other circumstances it would attract little of our attention, and the kind of breathing which belongs to it is often spoken of as being merely rough or creaking and is accompanied with some degree of crepitation. Again, local bronchitis is generally a suspicious sign, for it indicates local irritation.]

In certain cases, tubercles are deposited very rapidly, and at the same time very generally, being disseminated in small isolated granules throughout the entire of each lung. As a consequence of the rapidity of their development, bronchitis is set up; and because the deposit of tubercles is general, the bronchitis is general also. Moreover, as there is no aggregation of tubercles in any particular spot, none of the evidences of consolidation are presented. Here, then, the adjuncts to the bronchitis are wanting; and the case presents only the characters of one of general bronchitis. It is only as the disease goes on without showing any disposition to abate, and as other symptoms of a constitutional and local character are superadded, that you are enabled to assure yourself with certainty of the true nature of the case. I may, however, observe, that the difficulty in diagnosis affects chiefly adults, since persistent general bronchitis occurring in children is of itself strong evidence in favour of the presence of tubercles in the lungs.

The following example will illustrate the extent to which a case such as I have described may be deprived of the indicative circumstances so necessary for its diagnosis.

It occurred in the person of a young lady whom I attended with a surgeon in this town. Her age was nine. She was the last surviving member of her family, all her sisters having been cut off by consumption. Her health had been excellent up to the day of her fatal illness, which commenced as an ordinary attack of continued fever, entirely without any chest-complication. At the end of a fortnight, she seemed to improve, and her fever subsided; but it returned in two days afterwards with increased severity, and now was attended by evidence of irritation within the lungs, afforded by the presence of crepitation throughout the chest. It was at this time that I first saw the patient, and the remainder of her illness comprised only the short term of nineteen days. Her symptoms were those of high feverish reaction, without delirium of importance until the

close of the case. Perspiration was present at times during the day, and sometimes was profuse; at night, the skin was dry and hot. The pulse at first was 120; respirations 60; but each afterwards rose to 180 and 70 respectively. General crepitation continued throughout the chest; but there was neither local dulness nor prolonged expiration; yet there was very little cough; and, as she swallowed her expectoration, it was probably in small quantity. As the disease progressed, the lips became livid, hæmoptysis occurred, and delirium set in. I was not able to obtain an examination of the body; but, as the case developed itself, no doubt could be entertained that the bronchitis, which was the only disease present to our observation, depended upon a special cause affecting the entire lungs, and that such cause consisted in a general formation of tubercle.

I may just add, that Dr. Evans, who saw the patient with us, mentioned to me a very similar case which had occurred to himself, in which also phthisis ran an acute course without any cough at all, although the physical signs of bronchitis were present throughout.

In the cases of which I now speak, the tubercles are deposited rapidly; and the progress of the case is usually rapid in proportion, though rarely so rapid as in the instance I have just detailed. Hence these cases are termed acute phthisis; and, as I shall mention to you immediately, other difficulties may arise in the diagnosis than that of distinguishing the case from one of pure bronchitis.

Precisely the same remarks which I have now made in relation to the bronchitis of phthisis apply also to the fever which accompanies the deposit of tubercle. As is the case with bronchitis, the liability to fever may depend on extraneous circumstances; but most generally it depends on the degree of rapidity with which the tubercles are formed. Hence the serious significance of a feverish attack in one suspected to be disposed to phthisis, The history of many consumptive patients is made up of a succession of apparently trivial attacks of fever—attacks which, in another individual, would hardly be thought worthy of special care.

After such an attack, you will commonly find your patient's health occupying permanently a lower level; and you may be assured that the constitutional reaction has been the outward evidence of an addition having been made to the disease already present in the lungs.

But it is a very important circumstance, that like bronchitis, there is nothing in the fever of tuberculosis which of itself distinguishes it as belonging to that disease; its connexion is determined by collateral circumstances. The remark I made in respect to bronchitis, that we use comparison in forming our

diagnosis far more largely than we perhaps imagine, is especially true of fever; and, as fever is so common an occurrence, and attends so many different maladies, the possession of means for instituting such comparison is of still greater importance than it is in the instance of bronchitis. We are sometimes left with the fever as our only guide, and are then forcibly reminded of our need of further help.

I attended a little boy, aged six, with one of the most experienced surgeons of this town. His illness lasted for eight weeks; my attendance comprised the last week only. His sole symptom was persistent feverish reaction, though with scarcely any delirium; the one single symptom of a local character was constant rapidity of breathing. The child's chest was searched over and over again, both by my colleague and by myself, for any indication of disease; but, to the very last, percussion was normal; not even a wheeze could be discovered; and there was entire absence both of cough and of expectoration. Death was rather sudden—I suspect, from fainting. On examination, both lungs were positively filled with tubercles.

Not long afterwards, a second case, precisely similar as regards the entire absence of every symptom referable to the lungs, came under my care, also in consultation; yet the lungs were occupied by tubercles in every part. Here, however, there was inflammatory softening of the central parts of the brain; and symptoms of cerebral disease were prominent. Close attention was, however, bestowed upon the condition of the chest, for the purposes of diagnosis.

These instances exemplify another form of acute phthisis, in which, from the absence of explanatory indications, the fever at the outset of the disease is apt to be mistaken for typhoid fever.

But, after all, it is not of very frequent occurrence, that we are unable to diagnose the fever attendant upon the deposit of tubercle in the substance of the lungs, thanks to the assistance we mostly derive from the employment of auscultation and percussion. There is, however, another organ in which tubercles are liable to be produced, at least at an early period of life, and sometimes also in adult age, to which we have no means of applying physical examination. The cerebral membranes are often the seat of tubercle in childhood, and give rise to a most fatal affection. But the affection is also of a most insidious character. In the early stage of the disease, no characteristic symptoms of cerebral disorder develope themselves; the fever which attends the formation of the tubercles is alone present. Here there are no physical signs to aid us; and the diagnosis of the disease, previous to the setting in of decided cerebral symptoms, is confessedly a matter of great difficulty. "A few years ago," says Dr. Wilks, "I collected a large number of cases

of this affection in children, and in none of them was a correct diagnosis made during the first few days of illness, the case being styled one of fever or stomach-disturbance; and this error of diagnosis, or rather want of appreciation of the real nature of the case, is the rule." Dr. Wilks, however, draws attention to one distinctive characteristic, which may serve to arouse a suspicion as to the true cause of the fever; and I am sure that his observation will be found correct. He notices the irritability of the patient in the fever of acute cerebral disease, which contrasts in a most marked manner with the torpor of the fever-patient. When the disorders "are at their height," he observes, "and the patient is extremely ill, the fact is often very striking: the one with the inflammation of the brain being distressed by the slightest movement; it is irksome for him to be spoken to; he coils himself up in bed; is peevish, and wishes only to be left alone . . . . . whereas a patient with fever generally suffers nothing, and he evinces no trouble or alarm when touched."

Were we but able to apply to the brain the same method of physical examination we employ in cases of disease of the lungs, no doubt the proportion of doubtful cases of this disease would be reduced to the smaller proportion observed in the instance of pulmonary tubercle.—*British Medical Journal*, *Jan.* 28, 1865, *p.* 81.

16.—ON THE USE OF HOT WATER AS A REMEDY FOR PROFUSE PERSPIRATION.

By Dr. Robert Druitt, London.

If a part of the body that is perspiring be bathed with quite *hot* water till it becomes decidedly hot and *red*, the skin will become dry, and will continue so for a greater or less period of time.

If *cold* water be used, the part remains cool for some time and then becomes gradually warm or glowing; if *tepid*, it is usually made unpleasantly chilly and flabby; if *warm*, it is left perspiring; if *hot*, it is left hot, red, and dry.

The terms cold, tepid, warm, and hot are merely relative; what would be warm to one would be lukewarm to another person; but when I say *hot* water for our present purpose I mean water as hot as can be borne without pain. It may be used by sponging or immersion, and must be continued till the parts treated are hot, red, and *tingling* with heat—almost scalded, in fact. A good wipe with water at 130° is easily borne; for immersion the heat must be less; but the feelings are the only guide.

The circumstance which led me to recommend this remedy

was the observation of the painful dryness of a hot skin in feverish attacks, and of the dryness produced by using too hot water in a bath, and by the clumsy use of the lamp-bath, which may make the skin dry instead of moist if not well managed.

The cases in which I have recommended it with benefit are—first, those of general tendency to perspire to a distressing degree in hot weather, the patient being in good health. If a man who has thoroughly used a cold bath in the morning be obliged to change his shirt in the middle of the day, for example, he will keep his skin comfortably dry for a certain time by a good wash with very hot water.

The next class of cases are those in which, with or without tendency to perspire over the body generally, but most probably without, there is a tendency to distressing perspiration of some particular part; as the axillæ, hands, feet, &c. Patients who seek relief for this are generally young persons from thirteen to twenty, and they often endure great misery and persecution in consequence of this symptom, which really admits of easy medical treatment. The health is sure to be improved by free purgation, and by quinine, air, exercise, &c. But the distressing local symptom may be got rid of for hours at a time by the thorough application of the hottest water to the offending part until it is red, hot, and tingling as if scalded.

Thirdly, there are the cases of true hectic; diurnal shiverings, followed by heat, and drenching perspiration of an earthy, sickly odour, and depending (as we suppose) on absorption of decaying pus from some internal organ, probably lung, &c. In these cases I have tried every remedy I know of without result. I have caused profuse perspiration by the lamp-bath in the afternoon without preventing the access of hectic and perspiration in the evening; and confess my remedy inert—or next to it—in these cases.

But there is a fourth variety—the ordinary night sweat of the phthisical, not preceded by regular hectic paroxysm, but induced by all that relaxes, lessened by all that strengthens, and coming on when the patient falls asleep. For many of these cases the hot water gives relief, to a certain extent, especially if the perspiration begin, as it often does, on one special part of the body by preference, as the chest, hands, or feet. In such cases I have the testimony of patients that the hot water greatly helps to control the sweat. The way is, when the patient goes to bed, to have the chest reddened with hot water, and wiped dry. One patient, whom I see daily, and who is confined to his bed, calls out for it so soon as he perceives the dampness beginning, and has it used to chest, hands, and feet, and by this means often, not always, passes a night without being obliged to shift his linen.

In conclusion, let me say that I only offer this as a contribution towards the relief of an unpleasant symptom, and not as a cure for a disease; and that whoever uses it must recollect that it is not *warm*, but *hot* water, just below scalding point, that is to be employed.—*Medical Times and Gazette, March* 4, 1865, *p.* 222.

17.—ON THE AUSCULTATORY SIGNS WHICH PRECEDE CREPITATION IN PNEUMONIA.

By Dr. A. T. H. Waters, Physician to the Liverpool Northern Hospital.

Most authors since the time of Laennec have regarded the crepitating râle as the first recognizable sign in the pneumonic inflammation. Dr. Stokes has, however, asserted that an "intense puerility of respiration" precedes this phenomenon. "The physical signs of the first stage of pneumonia," he says, "are still to be determined with accuracy. Without possessing a sufficient number of observations to determine the point, I am led to the belief that an *intense puerility of respiration in the affected part* will be found to be the principal phenomenon. In cases in which inflammation was spreading upwards, I have often found that a puerile respiration preceded the crepitating râle for some hours; and that this was not a general but a partial condition was shown by its being much more intense in the vicinity of the disease than in the opposite lung. Indeed, in cases presenting great puerility of respiration with fever, we may often prognosticate the occurrence of the crepitating râle. Thus in a case in which the disease attacked both lungs as well as the pericardium, I observed this sudden appearance of intensely puerile respiration on three distinct occasions: in two it was followed by the crepitating râle and other signs of pneumonia, and in one it was removed by bleeding before the above signs had occurred."

I think objection might be fairly taken to Dr. Stokes's conclusion, founded on his observation, that a puerile respiration is heard in a pneumonic lung above the level of existing crepitation; for it might be said that near to the inflamed portion the lung would be doing greatly increased work, and thus we should expect to meet with a loudness in the respiratory murmur. Nor would such objection be fully met by his statement of the fact that the murmur was less puerile in the opposite lung than above the seat of crepitation. But the observation on which Dr. Stokes mainly depends in support of his view is the one he last refers to. In the case in question he met with the phenomenon on three distinct occasions, and in two of these unmistakable evidence of pneumonic inflammation followed.

To render the observation of this phenemenon, however, perfectly trustworthy, it must be made on a case, not where there is progressive inflammation, nor even where there are consecutive attacks of inflammation, but where, the lungs having been previously healthy, acute primary pneumonia comes on suddenly. Two such cases have recently occurred under my care, and have enabled me to confirm by observation the view which was originally advanced by Dr. Stokes.

Case 1.—Patrick F., a carter, was admitted into the Northern Hospital, under my care, on August 8th, 1864. On the day of admission, at an early hour, he was out in a shower of rain, got very wet, and did not change his clothes afterwards. In the course of two or three hours he felt pains about the limbs, and had severe rigors.

When admitted into the hospital about midday, he was seen by the house-surgeon. He then complained of pain in the lower part of the left side of the chest. There were no febrile symptoms, and no abnormal physical signs about the chest.

On the following day, about noon, his condition was as follows:—The pulse was 120, and full; respiration, 32; skin very hot and dry; tongue coated with a white fur. The pain in the side had increased. There was no cough, but much dyspnœa. There was no deficient movement of the left side of the chest, and the percussion sound was natural. *At the lower and back part of the left lung a loud, harsh, peculiar respiratory murmur was audible.* No such sound could be heard elsewhere. The patient was ordered a grain of opium three times a day, with small doses of tartar emetic.

The next day the pain in the side had almost gone. The pulse was 104; the respirations were 28. The physical signs were as follow:—Deficient movement of the left side; dulness at the left base, with crepitating râle over nearly the lower half of the left lung. The crepitating râle, which was distinctly of a pneumonic character, occupied in fact, this day the seat of the harsh, loud respiration of the preceding day.

It is needless to follow the history of the case further. The crepitation was succeeded by bronchial breathing and all the symptoms of confirmed pneumonia. The patient made a satisfactory recovery, and was convalescent on the eighth day of the attack.

Case 2.—A Frenchman was admitted into the Northern Hospital under my care, on Jan. 23rd of the present year. He complained of dyspnœa and pain in the chest. On examination, *a loud, harsh, respiratory murmur* was heard over the lower and back part of the left lung. The movements of the side were good, and there was no crepitation nor dulness. The breath-sounds over the opposite lung were normal. On the following

day the physical signs were as follows :—Slight dulness at the base of the left lung, and well-marked crepitation over about the lower half of the same lung. In fact as in the preceding case, the puerile respiration of one day was replaced by the crepitating râle on the next. The patient subsequently had all the symptoms of confirmed pneumonia—dulness, bronchial breathing, and rust-coloured sputa. He is now (Feb. 1st) convalescent from the attack.

M. Grisolle, in the recent edition of his work on Pneumonia, referring to the alleged puerility of the respiration in the disease, says that he has never met with it as an initial symptom, or as a precursor of the crepitating râle. He has, however, he says, frequently called attention to another phenomenon which he has observed in some cases of the disease—viz., a weakness of the reipiratory murmur. He has observed this, he says, on several occasions as a precursor of crepitation.

I have never met with the physical sign which M. Grisolle speaks of ; but of the existence of a loud, rough, harsh respiratory murmur, in the cases I have referred to, as an initial symptom of pneumonic inflammation, there can be no doubt. And although I would refrain from asserting that in all cases of pneumonia this harsh respiration precedes the crepitating râle, yet I cannot but think that the state of the respiratory membrane, at the commencement of the affection,— a subject to which I shall refer in a future paper on the morbid anatomy of the disease,— is just that which would be likely to produce the phenomenon. —*Lancet, Feb.* 11, 1865, *p.* 147.

18.—ON THE TREATMENT OF PNEUMONIA BY RESTORATIVES.

By Dr. John Hughes Bennett, Professor of the Institutes of Medicine, and Senior Professor of Clinical Medicine in the University of Edinburgh.

[During a period of six years and a quarter of active duty in the Royal Infirmary of Edinburgh, Dr. Bennett has treated 129 cases of acute pneumonia. Of these 105 were uncomplicated, and *all* recovered, although many of them were very severe. Amongst the twenty-four complicated cases were four deaths.]

In the 4 fatal cases, death was evidently caused by complications independent of the pneumonia. To arrive at true statistics with regard to treatment, therefore, it becomes necessary to eliminate them, and to fix our attention on the 125 cases which recovered.

Sex—Of the 125 cases, 85 were males and 40 were females.

Age—The average age of the males was 31½ years. The

average age of the females, 28½ years. The average age of both 30½ years. Between the ages of 5 and 15 years was 1 case—a girl; between 10 and 20 years, 29 cases—12 females; between 20 and 30 years, 35 cases—11 females; between 30 and 40 years, 23 cases—7 females; between 40 and 50 years, 24 cases—6 females; between 50 and 60 years, 11 cases—1 female; between 60 and 70 years, 1 case—a female; and between 70 and 80 years, 1 case—a female.

Simple or uncomplicated pneumonia.—Of the 125 cases, there were 105 simple or uncomplicated, and 20 complicated. Of the former, there were 74 males and 31 females. 79 were single and 26 double cases. Of these, I find that the clerk has omitted to state either the exact day of rigor or of convalescence in 6, so that no conclusion can be derived from them as to the duration of the disease. Of the remaining 99 cases, 73 were single and 26 double. The duration of the disease in the 73 cases of single uncomplicated pneumonia, counting from the occurrence of rigor to the commencement of convalescence, was as follows: 2 cases recovered in 5 days, 4 cases in 7 days, 5 cases in 8 days, 2 cases in 9 days, 8 cases in 10 days, 7 cases in 11 days, 7 cases in 12 days, 4 cases in 13 days, 13 cases in 14 days, 2 cases in 15 days, 3 cases in 16 days, 3 cases in 17 days, 3 cases in 18 days, 1 case in 19 days, 2 cases in 20 days, 3 cases in 21 days, 1 case in 22 days, 2 cases in 23 days, and 1 case in 26 days; the average duration, 13⅔ days. The duration of the disease in the 26 cases of double uncomplicated pneumonia, counting from the occurrence of the rigor to the commencement of convalescence, was as follows: 2 cases recovered in 8 days, 1 case in 9 days, 2 cases in 10 days, 2 cases in 11 days, 1 case in 12 days, 1 case in 13 days, 4 cases in 14 days, 1 case in 15 days, 2 cases in 16 days, 2 cases in 18 days, 2 cases in 19 days, 1 case in 20 days, 3 cases in 21 days, 1 case in 27 days, and 1 case in 55 days; the average duration, 16¾ days. Of the 105 simple or uncomplicated cases, there were 9 bled by venesection and subjected to an antiphlogistic treatment before or immediately upon admission, before I saw them. The amount of blood extracted varied from 12 to 36 ounces; the latter in two bleedings. The duration of one case is not stated. Of the remaining 8 cases, the duration was as follows; 1 case recovered in 7 days, 2 cases in 14 days, 1 case in 16 days, 1 case in 17 days, 1 case in 20 days, 1 case in 27 days, and 1 case in 55 days; the average duration was 21¼ days.

The average duration of residence in hospital of the single uncomplicated cases of pneumonia—excluding 2 cases in which the date of dismission is omitted, making 77 cases—was 21⅔ days: for the males (52), 18⅗ days; and for the females (25), 27⅕ days. Of the 26 double uncomplicated cases, the average

duration of residence in hospital was $23\frac{3}{5}$ days: of the males (20), $23\frac{17}{20}$ days; of the females (6), $22\frac{2}{3}$ days. The average duration of residence in hospital of 8 cases bled by venesection early in the disease (the 9th case being excluded in consequence of the day of dismission not being entered in the case-book) was 32 days.

Complicated cases of pneumonia.—Of the 20 complicated cases of pneumonia, 16 were single and 4 double. Of the 16 single complicated cases, the duration of the disease cannot be determined in 3. Of the remaining 13, the duration was as follows:—1 case recovered in 7 days, 2 cases in 9 days, 1 case in 10 days, 1 case in 12 days, 2 cases in 14 days, 1 case in 15 days, 2 cases in 16 days, 2 cases in 19 days, and 1 case in 48 days; the average duration, 16 days. Of the 4 double cases of complicated pneumonia, 1 case recovered in 9 days, 1 case in 14 days, 1 case in 15 days, and 1 case in 18 days; the average duration, 14 days.

The extent of pulmonary tissue involved in the pneumonia was carefully determined in each case, and the average duration of the disease in the 95 single cases, deducting the unsatisfactory ones, counting from the rigor to the commencement of convalescence, was as follows:—One-quarter of the lung (2 cases), average duration $8\frac{1}{2}$ days; one-third of the lung (12 cases), 12 days; one half of the lung (25 cases), $15\frac{3}{4}$ days; two-thirds of the lung (34 cases), 14 days; three-fourths of the lung (6 cases), $14\frac{2}{3}$ days; four-fifths of the lung (1 case), 12 days; the whole lung (15 cases), $10\frac{3}{4}$ days. Of these 95 cases, the right lung was affected in 58, the left lung in 37. Among these 95 cases also the pneumonia was confined to the upper lobe in 11 cases, or nearly 1 in 9 of the whole; and the average duration of the disease in these was 13 days, and of their residence in the hospital $14\frac{1}{2}$ days.

A careful study of the preceding facts will, I think, tend to establish some new truths and correct several prevailing errors with regard to pneumonia. I must remind those, however, who may yet be sceptical as to the value of a restorative treatment, and imagine that some of these cases might not have been pneumonia at all, that they were all diagnosed and treated publicly in the Royal Infirmary; were all examined, not only by myself, but by intelligent clerks and assistants; and were all made the subject of clinical lectures and commentaries at the bedside. There is therefore the positive certainty, not only that every one of these cases was a genuine case of pneumonia, but that no other cases of the disease but what are tabulated were treated by me during the period referred to. It should be explained, however, that a few cases were partly treated by my colleagues, either before I assumed duty or after I left it, in the too frequent rotations which occur among the clinical professors,

in this University. Such cases are not inserted. It is also necessary to point out that two or three cases brought into the house by the police in an exhausted condition died before I saw them, and are also not inserted. It is the more important to refer to such occurrences, because they serve to account for the differences which must always exist between general hospital and clinical statistics. Every hospital physician must be aware that the general records of the house afford no index whatever to the number of cases of acute pneumonia treated clinically, comprehending as they do not only consecutive, latent, and chronic pneumonias, but not unfrequently cases of pneumonia, which have entered in a dying condition and have not been treated at all.

1. The first great fact which the preceding figures serve to establish is, that a simple primary pneumonia, whether single or double, if treated by the restorative plan, is not a fatal disease. Surely 105 cases, of which 26 were double, are sufficient to establish this proposition, especially when it is considered that they were diffused over sixteen years, and occurred in all seasons. Amongst these, also, the whole of one lung was involved in no less than 15 cases, and the symptoms in many of them were exceedingly severe. Neither will any theory as to strength of constitution or change of type in disease explain the result, as several of the cases were those of healthy, vigorous young labourers, whilst others were those of weak and broken-down sempstresses. In any and every case the disease appears to go through its natural progress, so long as the body is not too much exhausted, and the physician as early as possible supports it by nutrients and restoratives.

2. As a general rule, prostration and weakening complications or remedies, not only materially lengthen the period of the disease, but especially prolong the convalescence. It is easily understood, therefore, how it happened that the antiphlogistic treatment of former days proved so fatal. The facts collected for me by Dr. Thorburn, from former case-books of the Royal Infirmary, prove that in weak cases a lowering treatment was still employed, though not, perhaps, to so great an extent as in robust persons.

3. It is generally supposed that the extent of the disease and the amount of lung affected must influence the result and duration of the disease. As to the result, all my cases recovered, even the 15 cases where the whole of one lung was involved, as well as the 26 cases where portions of both lungs were affected. In one complicated case, the whole lung on the right side, and and two-thirds of the lung on the left side, were simultaneously affected, thus leaving only one-third of a lung to respire with; and yet, without bleeding, and with the aid of nutrients and

restoratives, she was convalescent on the fourteenth day, and left the house quite well after a sixteen days' residence. With regard to duration, the extent of the disease does not exert so much influence as is generally supposed. If only a fourth of one lung be affected, the recovery may take place in eight days; but after that, whether the half or the whole of one lung, or two-thirds of both lungs be affected, it does not appear to cause much difference. Cases with half a lung pneumonic recovered in 15, with two-thirds of a lung in 14, with a whole lung in 10, and with portions of both lungs in 14 days on the average.

4. Since the observations of Louis, it has been supposed that a pneumonia at the apex of a lung was more fatal and more prolonged than one at the base; and so it may be with an anti-phlogistic treatment. But with a restorative treatment, the preceding facts show that in 11 cases where the disease was confined to the apex, recovery took place in all, on an average, on the thirteenth day.

5. As means of palliating symptoms, and especially pain and dyspnœa, warm fomentations and poultices I believe to be the best and safest. Chloroform has been given by Varentrapp and others with good effect. No doubt also small bleedings, to the extent of eight or twelve ounces, give relief; but in debilitated persons are dangerous, and in all tend, by weakening the strength at a period when the depressed system is struggling to regain its equilibrium, to prolong the convalescence and favour dangerous sequelæ. Still a small bleeding may be employed as a palliative with caution, to relieve engorgement of the lungs and congestion of the right side of the heart, although it is very rarely required. It should be remembered, in cases of double pneumonia, that there is often great dyspnœa on the sixth or seventh day, which will generally yield to warm poultices locally, and moderate doses of wine.

6. As a curative treatment I am satisfied that the best plan is rest in bed, nutritive drinks, especially good beef-tea, from the first, assisted by port wine—from four to eight ounces—if the pulse become weak, and solid nutrients as soon as they can be taken. The elimination of the exudation may be further assisted by salines (acetate of ammonia and small doses of tartar emetic one-sixth of a grain) and diuretics (nitric ether), although nature will accomplish this herself if the strength of the body be maintained. All active purgatives, contra-stimulants, depressants, anodynes, and lowering remedies of every description should be avoided.

After carefully studying all that has been written on the subject of pneumonia, as well as the numerous statistical tables of the results of various kinds of practice, I can only account for the remarkable success which has followed the restorative treat-

ment in my hands by supposing that acute pneumonia is not a fatal disease, if the strength be supported and there be no complication. The former idea of medical practitioners, that it was a dangerous disorder, and required active depletion and antiphlogistics to prevent its passing into the suppurative or fatal stage, was erroneous, and the result of the imperfect knowledge of pathology which then existed. Suppuration, so far from being fatal, is, as we have endeavoured to show, necessary to the resolution of the disease; and a fatal result, so far from being avoided, was produced to the extent of one in every three cases. The late Dr. Todd, while he supported, also stimulated to a great extent, and the result of his practice was a mortality of one in nine cases. In not one of my uncomplicated cases has there, I repeat, been a death for sixteen years, although 26 were double cases, and 15 were cases in which the whole lung was involved. Why, therefore, any such case should have died in the practice of Dr. Todd I can only ascribe to unnecessary stimulation, as that seems to be the only point in which his practice differed from mine.

In an able article in the 'British and Foreign Medico-Chirurgical Review' for July, 1858, it was endeavoured to be shown, from the contrary results obtained by statistics, that the result was governed by hygienic laws or peculiarities such as age, season, climate, &c. I consider that my cases prove the fallacy of such reasoning; and that, looking at the time over which they extend, as well as all the other circumstances which are adverted to, it might be easily shown that the uniform good results in my uncomplicated cases depend on some other cause. That cause, I can have no doubt, is simply supporting the patient by nutrients and restoratives from the commencement. It is the want of that support which explains the mortality in the practice of those who, while they do not actually lower their patients, fail to see that, in a certain proportion of cases, either the disease itself or excessive stimulation exhausts and proves fatal.—*Lancet*, *Feb.* 25, 1865, *p.* 195.

19.—ON RHEUMATIC BRONCHITIS.

It will be granted that in reference to treatment that it is a matter of very great importance to study the nature of tissue changes in disease. It is more important to know that certain thoracic symptoms depend on rheumatism even than to diagnose the exact position of the pulmonary disease. In Dr. Laycock's lectures, published a few years ago, much attention was paid to this subject. If the views of Dr. Jones be correct, we have, in certain cases of bronchitis, more rational grounds for treatment.

We can treat rheumatism by general remedies, as by the iodide of potassium and alkalies, with more hope than a more or less local disease like bronchitis, by expectorants or other empirical measures. We may refer to some cases recorded in our Hospital Reports by Dr. Sutton a few months ago, illustrating, in a very definite manner, the relations of rheumatism to certain local affections.

We must, however, keep in mind that rheumatism and chronic bronchitis are very common diseases, and their co-existence does not by any means imply a relation. Besides, it is a common and a useful practice to give iodide of potassium in chronic bronchitis, especially with emphysema, whether it be attended by rheumatism or not. Moreover, in Dr. Jones's second case the treatment was too complicated to enable us to judge what share the iodide had in the cure. The two cases here related can, then, only be considered as a text for remarks embodying the more general experience of a Hospital Physician on an interesting subject. We report Dr. Jones's remarks, in order that he may speak for himself.

Acute Rheumatic Bronchitis (under the care of Dr. James Jones).—A. E., aged twenty, was admitted into Gurney ward with acute rheumatic arthritis, sharp fever, scanty high-coloured urine copiously depositing pink lithates. There was also much dyspnœa and cough, with all the physical signs of capillary bronchitis throughout both lungs, but no carditic complication. The dyspnœa was generally most severe at night. She was treated with alkalies in large doses and morphia: the joints were enveloped in cotton wool. Under this treatment both the arthritic and bronchial affections steadily and *pari passu* subsided, and in three weeks she was able to leave her bed.

Chronic Rheumatic Bronchitis.—M. R., a man of middle age, was admitted an out-patient with cough, dyspnœa, and chronic arthritic rheumatism. There is nothing in the physical signs to distinguish his case from one of idiopathic chronic bronchitis, but the symptoms run a somewhat peculiar course. The dyspnœa and cough suddenly subside, and as suddenly become aggravated without any apparent cause. The chest symptoms do not appear to be in any way influenced by the state of the arthritic affection, but seem to run an independent course. Under the use of iodide of potassium, bicarbonate of potass, compound tincture of camphor, and decoction of senega, both the bronchial and arthritic inflammation were greatly diminished. The patient declares that no medicine he had ever before taken did so much to his cough and breathing.

Dr. James Jones in commenting on these cases observed that the influence of chronic constitutional diseases in the production of bronchitis or the effect of constitutional diathesis in

modifying bronchial inflammation, were subjects of great importance, and afford a field for inquiry which had been but little explored. As a part of this very extensive subject, he would direct attention to a form of bronchitis not by any means uncommon, yet requiring a special mode of treatment—the rheumatic. His attention was drawn to this form of disease by the large number of cases of chronic bronchitis in rheumatic subjects to be found amongst the out-patients, especially during the winter months, and the mode of treatment which afforded them the greatest amount of relief. In this disease clinical medicine demonstrates what pathology suggests—from their complex anatomical constituents—that the bronchi must be liable to rheumatic inflammation. There appear to be two forms of this disease—the one purely rheumatic, of which the first case affords an example; the other of a mixed character, produced by the modifying influence of rheumatism, or the rheumatic diathesis on ordinary bronchitis exemplified by the second case. The majority of cases of rheumatic bronchitis exist in the chronic form. The physical signs give no clue to its nature, as they are identical with the corresponding idiopathic form. But the irregularity of its course, the seeming caprice which rules its exacerbations and its subsidences, and its amenability to those remedies which are found useful in relieving the arthritic rheumatism will enable us to differentiate between it and the idiopathic disease. Like arthritic rheumatism, it is often aggravated by the warmth of the bed, the dyspnœa becoming so severe as somewhat to mimic a paroxysm of spasmodic asthma, being like it also, relieved by a copious expectoration. Dr. Jones has not yet had any opportunity of determining the nature of the structural lesion, or whether it differed anatomically from that which accompanied the idiopathic disease. The treatment which he had found successful consisted of a combination of the alkalies and opium in the acute form, with the addition of iodide of potassium and senega in the chronic; an underdress of flannel, and the use of a respirator, as a protection against those atmospheric changes which exercise so great an influence over rheumatism.—*Medical Times and Gazette, Jan.* 7, 1865, *p.* 10.

20.—ON THE VALUE OF CHLOROFORM IN THE TREATMENT OF ASTHMA.

By Dr. Hyde Salter, F.R.S., Assistant Physician to Charing Cross Hospital.

[The inhalation of chloroform is beyond doubt one of the most powerful methods of treatment of the asthmatic paroxysm that we possess. The most common fault of it is that the relief

afforded is transient only, and merely coextensive with the insensibility produced. In some cases however, when the insensibility passes off the asthma does not re-appear, and in some the relief is produced without any insensibility whatever.]

There can be no doubt, I think, that chloroform dissipates the asthmatic spasm by relaxing muscular contraction, just as it will dissipate hysterical contraction of the *rectus abdominis*, and thus disperse a phantom tumour, and that it acts through the general nervous system. But I have seen one case, which I shall relate, in which it seemed to act directly on the bronchial muscle. I conclude this to have been so because I think the effect was too immediate for it to have taken place *viâ* the circulation and the general nervous system: the first act of inspiration would be accompanied with a sensible relief long before the blood charged with the chloroform could have reached the nervous centres.

Patients and their friends have often asked me if there was not danger in giving such an agent as chloroform in the height of an asthmatic paroxysm. And truly, looking at the alarming state of semi-asphyxia to which the asthmatic paroxysm often amounts—the turgid face, the small pulse, the struggling respiratory muscles, the almost absolute standstill to which both respiration and circulation are brought—one would be apt to think that it would take very little more to stop both the one and the other, and that it was not exactly the condition for which to administer a drug having so depressing an influence on both these functions. I can only say, however, that I have given chloroform in the very agony of the worst attacks; that so far from fearing it under such circumstances, it has been to relieve the intensest asthma—that which nothing else would reach—that I have most given it, and that I have never seen any bad effects from it. Indeed, the immediate and direct effect of the chloroform is to remove that which is the whole cause of the asphyxial stoppage—the bronchial spasm,—and to set the pulmonary circulation free. No sooner does it enable the patient to fill his lungs than the loaded right heart disburdens itself, the lividity and venous turgescence disappear, and the pulse regains its normal volume. The intensity of asthmatic asphyxia, so far from being a reason against the administration of chloroform, is the great reason for its immediate employment. I grant that if the same amount of lung-stoppage depended on any other cause than bronchial spasm (at least on any cause that chloroform would not relieve) its administration would be highly dangerous.

I may add that my experience does not induce me to believe that the presence of valvular disease, or muscular weakness of

heart, adds anything to the danger of chloroform, unless these conditions exist to such an extent as materially to affect the circulation. I believe that chloroform may be as safely given to a man with an aortic bruit as to one without one, provided there be no *symptoms* proper. I believe the circumstance that determines whether chloroform shall exercise a fatal influence on the heart's action is, not the presence or absence of organic heart disease, but some idiosyncrasy of nervous organization.

Case 1.—F. E.——, aged sixty three, engaged in the practice of the law, a sufferer from asthma of many years' standing, first began to take chloroform at the end of 1861, two years and three quarters ago, most probably at my suggestion, to relieve the violent attacks from which he then suffered. The dose in the first instance was small, and the effect always satisfactory and immediate. Not more than fifty minims was given on each occasion, and probably it was some time before that dose was reached. It never failed. One effect noticed then, which has been remarked ever since whenever the chloroform is given in full doses, was that it always made him feel chilly when recovering from its influence. It was applied on a handkerchief twisted into a hollow cone, into which the chloroform was dropped. The time at which it was exhibited depended entirely on the time at which the asthma was sufficiently bad to require it, and that was generally in some part of the night. The effect was then, as it has been ever since, and is now, immediate; after a breath or two had been drawn the spasm was felt to be giving way, and within a minute it was gone; and consciousness was gone too.

At this time the effect was not only immediate but final; the asthma was over for that bout, and the quantity necessary to finish it off was certainly never more, as has been stated, than fifty drops, probably not more than thirty. After having had an attack of asthma cured by chloroform, he was in the habit of considering himself "good for a week."

Since that time the asthma has gradually changed its character, and the attacks, instead of occurring in paroxysms of great severity at long intervals, have occurred more and more frequently and in a milder and milder form. As a result of this the chloroform has been more and more frequently called into requisition, and, again as a result of this, has gradually lost its power. Thus the dose required to relieve the spasm has continually increased.

The quantity required at different times would be various; sometimes ten minims would produce a complete and marvellous effect—completely stop the asthma, and send him off in a tranquil sleep for two hours; at other times dose upon dose would produce no effect, or next to none. On one occasion he

took three ounces of chloroform between one o'clock in the morning and nine o'clock in the evening. The reason of the difference at different times seemed quite inscrutable. I have observed it in other cases.

From the beginning, the taking of chloroform was followed, many hours afterwards, by two symptoms—nausea, and a copious secretion of viscid saliva from the mouth. If, for instance, he took the chloroform at the usual time, say four o'clock in the morning, he would have no nausea at the time, nor in the morning on taking his breakfast, which was generally a most substantial one, nor at chambers. But almost immediately after leaving chambers, at half-past four P.M., as soon as his mind was relieved from the preoccupation of his duties, he became conscious of the nausea. On his way home it would become very troublesome, accompanied by this profuse secretion of saliva, which was so great that, to his infinite disgust, he had constantly to get rid of it in the streets. It was frothy and viscous, so that sometimes he had great difficulty in getting his lips clear of it. On reaching home the nausea was sometimes so bad that he could take no dinner; generally he could and did, and usually found that the act of taking food relieved the sensation. Sometimes, however, after having taken a good dinner it all came up again. For the rest of the evening he was *hors de combat*—could do nothing, sat in his arm-chair in a state of exhaustion with a basin by his side; and this would continue till it drove him early to bed at eight or nine o'clock. The next morning on waking, supposing he had no asthma or chloroform, he would still feel the nausea, but in a diminished degree, often, however, so much that he could take no breakfast. On reaching chambers all sense of nausea would vanish, and would not be felt till business was over; then it would be felt again, but in a still less degree. And so it would go on till the second or third day, the flow of saliva and the nausea both gradually decreasing, when they would both finally cease. If, however, he took chloroform the second night, the nausea at breakfast-time the next morning would be still stronger, and would last longer—go on an additional day.

Besides these effects of the chloroform, which may be called the primary and immediate ones, certain more remote results gradually developed themselves in proportion to the length of time that the habit had existed and the increased quantity taken on each occasion. These secondary effects of the prolonged use of chloroform, as shown in this case, were—

1. Insomnia.
2. Deafness.
3. Apathy.
4. Tremulousness of hands.
5. An increase of the asthmatic tendency.

The most strongly marked and the most distressing of all was the insomnia. It came on very gradually, and for a long time the cause of it was not suspected. The patient was habitually a good sleeper, and the wakefulness that gradually showed itself was a strange and unaccustomed thing. In spite of all he could do, the hour at which he fell asleep became later and later—two, three, four o'clock, till at last it was six or seven. Indeed, he may be said ultimately to have had no sleep at all; two or three hours in a week was all he would get, and it became a wonder both to him and to me how he could exist with so little sleep, and not only exist, but pursue professional labours involving a great deal of close attention and mental wear and tear. When he went to bed he lay perfectly tranquil; there was not the slightest restlessness, but it seemed as if all tendency to sleep was abolished, as if he had no sleep in him, and never should want to sleep again: he was tranquilly wide awake. Finding sleep hopeless, he spent the chief parts of his nights reading. Everything that could be thought of in the shape of a sedative was tried. At one time it was thought that Indian hemp was giving him sleep, but it turned out afterwards that it was the remission of the chloroform that *allowed* him to sleep, and not the Indian hemp that was *making* him sleep. The dependence of the insomnia upon the chloroform was discovered in this wise: The first exhibition of the Indian hemp was accompanied by a remission of the asthma, and therefore with a cessation of the chloroform. After it had been given a night or two it was observed that the patient was sleeping a little towards morning, and night by night the time at which he fell asleep became earlier and earlier, till at last he would be so sleepy at ten o'clock (or even nine I think) that he could not keep himself awake, but was obliged to go straight to bed. The Indian hemp had been given early, at seven o'clock, in order that its influence might come on the sooner; it was now, therefore, given later, just before bedtime, but so great did its soporific effect appear to be, so immediately did an irresistible sleepiness come on, that the dose was reduced, and great was the credit that the "bang" received as an invaluable hypnotic. The sleep was everything that could be wished, and the neck of the insomnia was supposed to be broken; the Indian hemp had done its work, and was left off. But now came asthma, and with it chloroform, and within a few days the insomnia difficulty re-appeared; sleep came on later and later. The "bang" was again resorted to with confidence, but this time without success; the sleeplessness became worse and worse, till it was as bad as it had been before the hemp was given. The murder was now out; it was clear that it was the chloroform that produced the insomnia; that its former cure had depended on the remission

of the chloroform, and not on the administration of the Indian hemp, and now that the chloroform was keeping it up, the hemp was powerless. It was very curious to see, as the chloroform continued to be given, the gradual way in which the time that sleep would come on was postponed, just as previously, when the chloroform was left off, the time at which sleep supervened was a little earlier each night. I may here mention that, in confirmation of this view, the return of sleep has again followed the remission of the chloroform. My patient has now, at the time that I am writing these notes, left off the chloroform for twelve days, and he has, in the same gradual way as before, recovered his power of sleep, and this time without the use of the Indian hemp. I am not aware that this tendency of the protracted use of chloroform to produce insomnia has ever been noticed before.

Another effect was *deafness*. My patient has long been a little deaf but after he had taken the chloroform for some time this deafness was greatly aggravated, and became a matter of much inconvenience to himself and his friends. His mind also seemed to be affected; his intellectual powers were as good as ever, but he seemed to have lost the disposition to use them, and he would sit for hours in a sort of apathy, neither writing, nor reading, nor conversing, although naturally a man of great mental energy. He noticed, too, that his hands were tremulous, and that he had not his customary steadiness in delicate manipulations with them.

There cannot be a doubt, too, I think, that although the chloroform never failed to relieve the asthma at the time, it tended to increase the asthmatic tendency in the long run, and this probably by rendering the nervous system more susceptible, shaky, and irritable. Certain it is that the longer the chloroform was given, the more frequent and obstinate did the asthma become, and the more transient was the control that the chloroform exerted over it. Slighter things, too, would bring it on: the patient has told me that he is sure that an attack of cough which, now that he has left off the chloroform, has no power to bring on the asthma, would infallibly have done so at the time he was taking it in such quantities. At that time, too, phlegm coming away would secure no immunity. A good deal of this "induced" asthma, however, was not "the genuine article," but a spurious imitation. I was sent for one night in great haste to see him, and was told of the immense quantity of chloroform that had been taken, and the persistent recurrence of the asthma in spite of it. But on closely watching my patient I soon discovered that what he was suffering from was not true asthma at all; there was no real distress about the breathing, and I believe no narrowing

of the bronchial tubes. The differences between this "induced" and true asthma are, that it is rather a rapid and panting than a tight constricted breathing; that though loud, it is unaccompanied with wheezing; that it will suddenly cease spontaneously, remain absent from two and a half to three minutes, and then suddenly re-appear; that whenever he speaks it ceases, or whenever anything emotionally excites his attention. On the occasion in question I refused to give any more chloroform for the mitigation of this spurious dyspnœa, and in a quarter of an hour it was gone. I believe that by keeping up the chloroform condition it might have been indefinitely protracted. From that time my patient and his attendants have been able to recognize the difference betwen true asthma and this spurious dyspnœa. And the recognition of the difference is practically important; as the chloroform, although so effectual in relieving the true asthma, is not only useless against the spurious form, but inevitably keeps it up.

Such in this interesting case, were the secondary results of the prolonged use of chloroform. They are such as I think offer a very serious bar to the further use of the drug. It was impossible to observe the shattered condition of my patient, and not feel that the further pushing of the remedy would entail a worse condition than the asthma; that of the two evils, it would be the choice of the greater. And, with this case in view, I should, on any future occasion on which I might advise the use of chloroform, look forward to the possibility of having to prohibit its further employment, lest a worse thing than asthma came upon my patient. Chloroform-taking is like other dram-drinking, gains the same hold upon the patient, and leads to the same results. Indeed it is impossible not to see, in the case that I have related, a very close resemblance between the symptoms and those of chronic alcoholism—the vomiting, the hand-tremor, the mental enervation, the insomnia. It is a good rule, and of universal application, that we should be slow in ordering the habitual use of any remedy whose ultimate effects are deleterious, especially in cases in which the dose is likely to become large. And just as I should be chary of commencing the habitual use of alcohol or opium in any case of a chronic nature, so should I that of chloroform.

Case 2.—L. B., a lady aged forty-nine, who has long been a victim to asthma, is accustomed to get relief from her symptoms in the following way:—About seven or eight o'clock in the evening, when she feels she could go to sleep, she pours ten drops of chloroform on a pocket-handkerchief, and inhales it. In two minutes her asthma is gone, and, partly probably from the cessation of the fatiguing efforts of breathing, and partly from the effects of the chloroform, she is sound asleep; sleeps,

perhaps, for six hours, and wakes breathing quite freely. On one occasion, when I was called to see her in a very bad attack, knowing that so small a dose of chloroform as ten minims would stop an attack in the way I have described, I determined on administering it, in the confident expectation of giving her immediate relief. What was my surprise, however, to find that, although each dose that I administered gave her temporary ease, as soon as the effect of the chloroform passed off the asthma returned. I continued dosing her for about an hour, having given her in that time three or four drachms; and, when the effect of the last dose passed off, her asthma returned as severely as before I had administered any. It was quite clear that I was gaining nothing, and that for permanent relief the chloroform was worthless. I therefore desisted.

Subsequent experience has cleared up the mystery, and shown that chloroform only gives her permanent relief when it sends her into a continuous sleep; and that it only sends her into a continuous sleep when she takes it at such a time as she would, if free from the asthma, be likely to sleep continuously—as, for example, near her ordinary bed time. Given at any other time, it produces just as much momentary relief; but the state of sleep being transient—the chloroform sleep not passing into natural sleep—the asthma returns with the consciousness. This curious fact is consistent with another fact in this lady's case, and probably depends upon it—that she never has asthma when she is asleep, and that if she has asthma and can in any way get to sleep her asthma issure to cease; so, on the other hand, she is never awoke by asthma, like other asthmatics, but wakes free, her morning asthma appearing *immediately after* she is awake, and the time that it comes on depending entirely upon the time she wakes. The correct interpretation, therefore, of the cure of this lady's asthma by ten minims of chloroform is, that the chloroform temporarily relieves the asthma and sends her to sleep, and the sleep prevents the recurrence of the asthma.

I have repeated the chloroform experiment in this case on several occasions, and always with the same result. Any quantity is valueless to give more than a transient relief, unless given just at the time when sleep is disposed to supervene; but given then, ten minims is just as potent to cure an attack now as it was formerly.

The incompatibility of sleep and asthma which characterizes this case is quite peculiar; indeed, it is the very reverse of what one ordinarily sees, sleep being commonly one of the most powerful predisposing conditions of asthma. However, it is a peculiarity that is very fortunate for the possessor of it, for it

evidently imparts to chloroform, in her case, a power that it would not otherwise possess.

Case 3.—The following short notes of the effect of chloroform in one of the most distressing and intractable cases of asthma I ever saw I received only a day or two ago :—"And now about chloroform. I can hardly tell you much about it, as I have tried none since I left Surrey-street; and there, and elsewhere before, I only tried it once or twice at a time. It struck me, however, that its bad after-effects were greatly in excess of the short relief I obtained by the stoppage of the paroxysm. Latterly I inhaled from half a drachm to forty minims during the height of a paroxysm. In a few minutes I began to feel queer, but a delightful sort of queerishness; the paroxysm disappeared, and I could lie back half asleep, and breathing, as it appeared, naturally. But this effect wore off in half an hour or so, and the paroxysm would begin again, aggravated by a very distressing sort of feeling. I have not tried it in repeated doses, the first after-effects being so bad; in fact, it acts very like any preparation of opium does on me, but quicker, and the relief does not continue so long; the paroxysm returns sooner, and the distress is greater than after opium; but it does not make me so unwell the next day."

Case 4.—A medical man, formerly a patient of mine, and in whose case chloroform was the only thing that gave any relief, thus describes, in notes that he sent me, the effect that it produced on him, and the method of its administration :—"The effect of the chloroform was not immediate in subduing the paryoxysm, but was instantaneous in giving temporary relief by relaxing the spasm of the muscles of the bronchial tubes; but if I ceased its inhalation the symptoms returned in unmitigated force. As to the time I had to continue its use, that depended upon the severity of the paroxysm; and as to the quantity, I can scarcely give you a correct estimate, as Mrs. H. poured about a teaspoonful upon a handkerchief, and this was repeated as soon as the chloroform had evaporated, and continued until the urgency of the symptoms was removed. The effects were always accompanied by an acceleration of the pulse; and after the dyspnœa was relieved I usually went off to sleep."

I might multiply such narratives as these to a great extent, but I have given enough to show the value and efficacy of the remedy, and to illustrate some points of its physiological action.

On the whole, my experience of chloroform induces me to conclude—

That it holds a high place amongst the remedies of asthma; that there is probably no one agent that relieves in so large a number of cases.

That it operates with very various completeness in different cases.

That even where it does not *cure*, it is of great value by a temporary respite.

That no amount of asthmatia dyspnœa or asphyxia is any bar to its use.

That, if given constantly, however, in large doses, for a long period, a state of things arises which does, in my opinion, constitute a bar to its continuance.—*Lancet, Nov.* 5, 1864, *p.* 515.

21.—ON THE USE OF THE LARYNGOSCOPE.

By Dr. E. H. Sieveking, Physician in Ordinary to the Prince of Wales, and to St. Mary's Hospital.

Before speaking of laryngeal pathology as exhibited by the laryngoscope, let me say a few words anent the apparatus to be employed, the method of using it, and the points to be observed.

What do you want to do? In plain English, you want to look round a corner into a dark hole. This sounds paradoxical, but the paradox is solved by the laryngoscope.

The light either of the sun or of a lamp is received on a mirror attached to the observer's forehead, from which it is reflected on to a small mirror which is introduced into the fauces of the patient, and from this mirror again the light is thrown down upon and into the larynx. The angle of incidence being equal to the angle of reflection, you have merely to place your mirrors in such a relation to one another as to secure the proper direction of the rays of light, and a perfect and distinct image of the parts illuminated will be visible on the small mirror.

The sun, at all events in London, cannot be relied upon at any hour of the day; and patients would not, even if the sun were always shining, present themselves when his rays penetrated a given locality. We therefore find it much more convenient to trust to artificial light altogether, and accordingly have recourse to a gas moderator, or, as you see here, a paraffin oil lamp, which are not amenable to the caprices of the weather.

It is well to darken the room in which you make your examination; but even this is not absolutely necessary if the light is protected by a convex mirror on one side, and concentrated by one or more lenses on the other. Various complex apparatuses have been devised for the purpose of intensifying the light. What you see here is an arrangement for which I received the suggestion from my friend, Dr. Johnson, and which has been most satisfactorily executed by Messrs. Elliot and Co. The

lamp is a paraffin oil lamp, which gives a brilliant white light; on one side is a convex mirror, which reflects the light upon the lens opposite to it, and this, an ordinary bull's-eye, concentrates the light upon the reflector. The lamp should be placed close to the patient's head, on either side most convenient to the observer, and so that the lamp and the patient's and observer's heads are in the same horizontal plane. The patient, sitting erect, should push his head back so as to straighten the neck and facilitate the introduction of the small mirror.

Before attempting to do this, the observer should first ascertain that the reflector is properly adjusted. If he finds that the mouth is fully illuminated, he may fairly conclude that he will be able to throw the light upon the fauces. Beginners generally find a little difficulty in adjusting the reflector, but this is speedily surmounted. Whether you place your mirror on your forehead, as recommended by Dr. Johnson, or whether you prefer to use it as I generally do, is a matter of little moment. Try both ways, and adopt the one you like best. The mirror I employ has, as you perceive, a central uncovered spot. It is an improvement upon the original mirror employed by the father of laryngoscopy, Professor Czermak, of which I show you a specimen. The one I recommend has a small universal joint, and, being placed a little distance from the eye, is much easier of adjustment. I admit that with Professor Czermak's instrument I had a difficulty in seeing through the central orifice, but with this modification I find it advantageous first to adjust the reflector so that I am enabled to see the fauces through the opening with the eye covered with the reflector, and then I am certain to have the proper axis for both eyes. After you have satisfied yourselves that the lamp and the reflector (which is fixed on the head by an elastic band passing over the forehead) are in the right position, tell your patient to open his mouth widely, to protrude the tongue, and to breathe freely. Some patients will permit you to see into their larynx without in any way fixing the tongue, but this is the exception. As a rule, it is necessary that the patient, or the observer, should take hold of the tip of the tongue with a handkerchief or towel, to prevent this "unruly organ" from slipping back. Without this precaution you very often fail in your endeavours to see below the epiglottis. Your next step, after having got your patient in the proper position, is to warm the laryngeal mirror. This is necessary, to prevent the vapour of respiration being precipitated on, and dimming the mirror; but take care not to overheat it. As a precaution it is well to test the temperature by applying the back of the mirror to your own cheek.

Take hold of the stem of the mirror as if you were holding a

pen; introduce it into the mouth without touching the tongue, teeth, or lips, sinking the hand at first, and then raising it gradually so as to allow the mirror to form a curve until it reaches the uvula. Almost the whole secret of the manipulation in laryngoscopy consists in this. If you touch the lips or teeth you teaze the patient, but if you touch the tongue you are certain to excite reflex action, and the root of the tongue will arch up to impede your view. *Audacem fortuna juvat.* Push your mirror, after having passed the janitors, well against the uvula, and you will rarely meet with any serious impediment in getting a proper view of the larynx. An unsteady hand creates difficulties which need not exist, by bringing the mirror into contact with other parts than the uvula and soft palate. These are not very sensitive, but if you tease the root of the tongue or the arches of the palate, you infallibly excite reflex action, which will necessitate the withdrawal of the instrument. Having placed your mirror at an angle of about 45° with the horizon, it is well to rest the little finger of the right hand against the patient's cheek, by which means you will steady the instrument. You then secure a proper illumination of the laryngeal mirror, and by the necessary manipulation, which practice alone can teach, you successively examine all the parts exposed to view.

In making the examination and recording the facts observed, you must remember that you are looking into a mirror, which represents the parts in a different relation from their real position. The epiglottis, which in nature is turned from the observer, is represented as open towards him; the base of the tongue, which is in front of the epiglottis, appears in the mirror behind it; and the vocal cords equally occupy in the mirror a reversed position from that which they really possess. As laryngoscopic illustrations represent the appearances as seen in the mirror, it is necessary to remember the true relation in order to understand the drawing correctly. The lateral relations will cause less difficulty than the antero-posterior relations. What ordinarily appears to the left or right in the subject of observation placed before us, still continues so. We readily make the necessary allowances here from the habit engendered by custom; but it is different in regard to objects placed in front of each other and reflected in a mirror, because for this our daily life offers us fewer precedents. Perhaps the easiest way to realise the position of the parts as seen in the mirror is to imagine yourselves looking at the epiglottis and vocal cords through a hole in the cervical portion of the vertebral column. These points will become more apparent if we briefly examine the diagrams and preparations which I have placed before you. To the beginner I would say, Make your first experiment upon

a case in which you are informed there is no special difficulty; remember the rules laid down; bear in mind the anatomy of the parts and the direction in which you wish to carry the light; and, with a light and steady hand, you will scarcely fail, after one or two attempts, to see that which is ordinarily visible by the aid of this instrument.

Now, what are you look for? The first point that always attracts the attention of the observer, after having got the light and the mirror duly placed, is the epiglottis. It is the Cape of Good Hope of the laryngoscopists, and it is at times a difficult matter, a very difficult matter, to round the Cape. You will find that it varies as much in form as the nose, and its position not unfrequently is so prone as to render a good view of the subjacent parts almost an impossibility. Then, too, in irritable persons it undergoes a variety of contortions and contractions, which give it a character for muscularity greater than anatomists show it to possess. Still, as a rule, it serves merely as a landmark anatomically and pathologically: anatomically, because its well-known relations tell you in what direction to look for more important organs; pathologically, because the appearance of the mucous membrane covering the epiglottis is often a valuable indication as to the state of the subjacent parts. Examine the epiglottis carefully, the form, the colour, the attachments. It should be perfectly smooth, of a pale, yellowish-rose hue, and symmetrical. Look at the fossa formed by the front of the epiglottis with the base of the tongue, and note the state of any secretions upon or near the epiglottis. In disease you will see its colour varying from the dull white of anæmia to every shade of uniform, or streaky and patchy redness. It may present ulcers of varying size and depth; it may be deformed by old cicatrices or congenital malformation; tumefaction from inflammatory or œdematous thickening of the mucous and submucous layers may present itself; and it may exhibit extravasations of blood, or be more or less bathed in pus, or covered with a mucous secretion. Similar conditions may be discovered in the glotto-epiglottid fossa or on the glotto-epiglottid folds. In order to see the parts subjacent to the epiglottis, you will have to depress the handle of your mirror somewhat, so as successively to illuminate the posterior surface of the epiglottis; the arytenoid cartilages, with the corpuscula Santorini and Wrisbergii; the superior thryo-arytenoid folds, or the false vocal cords, as they are also called; and by turning the mirror laterally you will examine the right and left sides of the introitus laryngis, and especially the state of the ventricles of the larynx. These are cul-de-sacs inteavening between the vocal cords proper and the superior thyro-arytenoid folds. The state of the ventricles has an important bearing on the production of the voice,

inasmuch as their patulousness is essential to the free vibration of the vocal cords and the due production of voice. If the mucous membrane of the ventricles is swollen, the pitch and sonorousness of the voice is interfered with, and the more the pouch is obliterated the greater will be the interference with phonation. It is here that various secretions form and accumulate and you will readily understand why these should more or less affect the voice, as they present obstacles to the passage of the air, or diminish the vibrations of the aërial pulse. If the secretions are viscid, you may at times be puzzled by strings of mucus extending across the entrance of the larynx, closely simulating the vocal cords themselves. Little patches of secretion may also simulate ulcers. The sponge or brush will readily remove such secretions, and show the condition of the subjacent membrane. The ventricle, moreover, as in the case of E. G., whom I have already exhibited to you, and whom you will have further opportunities of examining, are the frequent seat of morbid growths. All these points having been noted, you seek the vocal cords themselves.

In a healthy larynx the vocal cords stand out, with a clear pearly sheen which is peculiarly characteristic. Once seen, the appearance is not to be forgotten; and you will often, in doubtful cases of laryngeal disease, rejoice at recognising this striking feature, because it will prove that the most important part involved in phonation (so far as the larynx is concerned) is healthy. The vocal cords, or inferior thyro-arytenoid ligaments, are mainly composed of yellow elastic tissue, but are endowed with the most marvellous capability of minute vibratile adjustment, subject to the controlling power of the will, exercised through the arytenoid, thyro-arytenoid, crico-arytenoid, and other muscles. It has been calculated that no less than one hundred muscles are brought into action in the ordinary modulation of the voice, but the note which is uttered depends upon the exact degree of tension of two ligaments, at the utmost seven lines in length, which is mainly determined by the two sets of muscles mentioned. You will have a measure of the minuteness of this adjustment when you reflect that a practised singer is capable of uttering three hundred different notes at will, for each of which on this minute vibrating cord a different stop must be applied.

The vocal cords are covered with mucous membrane, distinguished from the mucous membrane of the rest of the larynx, which is ciliated, by being squamous. The mucous membrane overlies the elastic tissue of the cords, and is liable—though to a much less extent than the mucous membrane in the vicinity—to congestion and the various morbid changes which are seen in this tissue elsewhere. An accurate knowledge of the anatomy

and physiology of the parts will enable you more fully to appreciate the importance of minute shades of difference in the appearance of the parts. A roughness of the surface or a discoloration which would lead to no palpable results elsewhere, here affects the comfort, the occupation, the life of the patient, and is therefore well deserving the study of the practitioner by any additional physical means that may be placed at our disposal. Here, too, we have a good illustration of the difference between vital morbid conditions and the condition of the same parts as seen after death : a difference that you should always bear in mind, as you may otherwise easily be misled into a wrong interpretation of the phenomena presented on the post-mortem table. If you had merely seen the interior of a larynx removed from the body, you would scarcely anticipate the marked contrast that exists in life between the vocal cords and the adjacent parts, nor would it be possible satisfactorily to determine the mode in which the variations of sound are produced by the vocal cords. You will find that there was much uncertainty as to the theory to be adopted regarding vocalization even in Müller's time; and it was not until after the practical introduction of the laryngoscope by Czermak that the study became satisfactory and the conclusions definite. If I name Czermak, it is not because I do not appreciate the labours of others in this field; but whatever others have done, he certainly has compelled us all, by the demonstration of the comparative facility of laryngoscopic examination, to make it a part of our medical studies.

It would be an injustice, in speaking of the subject, not to mention the name of M. Garcia, a well-known singer and amateur physiologist, who established by laryngoscopic examination much that is now known as to the physiology of the larynx before Czermak had made known his method; but the pearl that M. Garcia discovered was not appreciated by our profession, and therefore, so far as the medical world are concerned, laryngoscopy remained an unknown quantity until the appearance of Czermak's monograph.

It is not my object at present to do more than to interest you in the practical employment of the laryngoscope in the recognition and treatment of disease; therefore I do not attempt to lay before you an account of laryngeal physiology—a branch of science which is capable of further development, and which some of you may feel called upon to promote. Allow me yet to revert to a few points connected with the pathology of the larynx, to which I would draw your attention, as illustrating the value of this mode of investigating disease. The practical examination of the numerous cases that present themselves in our hospital will serve to impress upon your memory and

comprehension more vividly what I now merely show you *veluti in speculo.*

I have spoken of various morbid changes seen in or near the vocal cords, showing increased or diminished vascularity, congestive or œdematous swelling, ulceration, cicatrices, growths, all of which I have myself seen. If you consider the muscular and nervous functions of the parts, you will expect to see these also materially affected by disease. The *plus*—evidenced by spasm—is not likely to be very visible, because it will not leave you time for anything but immediate action to relieve your patient; but the *minus* of paralytic conditions is frequently observable in the irregular action or want of action of one or both vocal cords. It is here that stimulation, and notably the direct application of galvanism, is often of palpable benefit. And you will not examine many larynges before you will satisfy yourselves of the perfect facility with which you may direct the galvanic current, as well as any other medicinal application, to any given part of the larynx. This of course can only be learned by practice, and when the patients are before you; therefore I confine myself to exhibiting some of the apparatus suitable for topical applications, and leave the detail of treatment to clinical demonstration.

I have not, however, quite done yet. You may see further than the vocal cords. Their under surface can at present only be examined when there is a hole in the trachea, and at least one instructive instance is on record where this mode of exploration was practised with much benefit to the patient. But these are refinements of practice upon which it is unnecessary to dwell. I now merely speak of the ordinary employment of the laryngoscope, and I wish to remind you, that having explored the entrance to the larynx and the vocal cords, you should examine, as far as may be, the trachea. It is generally easy to recognise several rings of this tube, and you may, when the larynx is capacious and the patient steady, penetrate to the very bifurcation of the trachea. The only morbid condition that I have definitely recognised in the trachea, have been scattered ulcers; but it is manifest that, especially in those cases in which foreign bodies have slipped through the glottis, the discovery of their exact site by the laryngoscope may, as it already has been, prove of great practical value to the surgeon.

There is no better mode of initiating yourselves into the practice of laryngoscopy than to examine your own larynges, or those of your fellow-students. By this kind of exercitation you will familiarize yourselves with the use of the instrument no less than with the healthy condition of the parts. M. Garcia's interesting observations were entirely the result of examination made upon his own vocal cords, and as there is room for a

further cultivation of this field of physiology, each of you may be enabled to advance science in this direction. But apart from this, the mere dexterity of manipulation will be increased by the practice recommended, and if you know from experience in your own persons how to behave, you will more readily advise your patients what to do, and sympathize with their difficulties. Not every one, however, is a suitable subject for autolaryngoscopy: the narrowness of the introitus laryngis and the prone position of the epiglottis, no less than an unusual irritability of the parts, frequently render the process extremely difficult. Various methods have been suggested for the removal of undue irritability, such as the inhalation of small quantities of chloroform, the application of bromide of potassium, or the use of astringent gargles. Every now and then, even after you have acquired sufficient dexterity in the use of the instrument, you will meet with cases which present insuperable difficulties. You are then no worse off than your predecessors were without the laryngoscope. You will have to fall back upon those other symptoms which your knowledge of physiology and pathology will teach you to appreciate, and which this instrument is not intended to supersede.—*Lancet, April* 8, 1865, *p.* 360.

DISEASES OF THE ORGANS OF DIGESTION.

22.—PAIN IN THE STOMACH FOLLOWING INGESTION OF THE FOOD, SUCCESSFULLY TREATED BY MANGANESE.

By Dr. Arthur Leared.

Dr. Arthur Leared observes that pain caused by food is a very common functional affection of the stomach, and it affects women more frequently than men. Its ordinary position is at a spot just below the ensiform cartilage, but it may be localized at other points over the region of the stomach. At times it is diffused over a considerable space. When it has continued some time, and especially if its seat is the spot first mentioned, the affected part feels sore on pressure, and this soreness may remain after cessation of pain. In many cases pain extends a good way upwards beneath the sternum, more rarely it is diffused downwards towards the umbilicus, or even below it. It frequently extends towards the right in the track of the duodenum, and is sometimes experienced in the cardiac region itself. In many cases pain shoots from the part locally affected as from a focus, in various directions through the thorax, frequently to the upper part of the back.

The perception of pain by different individuals is variable, which partly accounts for the varying descriptions of the present

affection. By some patients it is described as a dull continuous pain, by others as of a tearing, gnawing, or scraping nature, or like that which might be caused by a tight ligature.

The pain usually comes on from a quarter of an hour to an hour after a meal, but in severe cases is induced by taking even a few morsels of food. Its duration is uncertain, but it usually lasts some hours. There appears to be always a short interval between the time of swallowing the food and the occurrence of pain. A valuable means of diagnosis between it and the pain of ulcer of the stomach, in which pain generally happens immediately on swallowing solid food, is thus afforded. But the intensity of pain affords no measure of the gravity of the disease. The pain of cancer or of other organic diseases of the stomach may be less severe than that which is purely of a functional nature. Flatulence and a sense of distension are experienced in some cases, while in others they are entirely absent. Constipation is not a prominent feature of the disease.

The pain and tenderness are seldom connected with gastritis, as their transient nature, as well as the accompanying symptoms, sufficiently prove. They are simply an expression of exalted sensibility of the mucous membrane of the stomach, which becomes intolerant of the natural contact with the food, or else of the gastric juice itself. The facts that the pain does not come on until the food has been some time in the stomach, and that albuminous food—that which requires gastric juice for its reduction—causes suffering, while starchy aliments cause it in a less degree, or not all, support the idea that the gastric juice is its source. I have been led by several circumstances to the belief, that the epithelial coating of the gastric mucous membrane is imperfect; that it is either shed too rapidly, or, owing to its imperfect growth, is inadequate for the protection of the delicate surface which it covers. Thus, the state of the tongue whose surface is continuous with that of the stomach, is generally very characteristic of the disease. Owing to a denudation of its epithelial covering, its extremity is very red, and its irritable-looking papillæ stand prominently out. The effect of treatment also proves that inflammation is not concerned in the disease.

I have thought it necessary to describe in some detail the nature of the disease before bringing forward a new remedy for it, because there are other painful conditions of the stomach which are not benefited by the same means. Its successful use will, therefore, greatly depend on accuracy of diagnosis. The gastric pain caused by gout, generally connected with an excess of acid, is not removed by the same treatment; the pain which attends organic disease of the stomach is little influenced by it;

and the same may be said of the neuralgic pain which is especially apt to occur when the stomach is empty.

In the treatment of the disorder before us, direct sedatives seldom give more than temporary relief. Opium has the great disadvantage of inducing constipation. Prussic acid, with or without alkalies, so useful in some kinds of stomach pain is generally futile. But the nitrate of bismuth has long been in deserved repute; more recently it has been almost superseded by its carbonate. These preparations are also open to the objection that they induce constipation. Both these substances are but sparingly soluble in the fluids of the stomach, and it occurred to me that their beneficial action may be exerted in a mechanical manner. It seemed possible that the diffusion of an inert powder over the walls of the empty stomach might, as it were, blunt the over-sensitiveness of its mucous membrane. With this view I tried, in several cases, the effects of silex prepared by precipitation from its solution, a perfectly inert and insoluble powder. The results were most encouraging. Passing then to substances which combine mechanical with a special action, I tried in many cases the saccharated carbonate of iron with little success. A fair trial of the magnetic oxide of iron yielded no better results. All the substances mentioned were given in doses of from x. to xx. grains. In pursuance of the same inquiries the black oxide of manganese, freed from impurities, at length came under trial. Here the results have been unexpectedly satisfactory. After having used it in several hundred cases, both in hospital and private practice, and after an experience of some years, I do not hesitate to pronounce it a most valuable addition to our stomach remedies. It is certainly more efficacious in allaying the hyperæsthetic state of the mucous membrane than bismuth. This has been proved by alternately exhibiting each remedy for a week at a time in severe cases, and carefully noting the results. But, independently of this, manganese has one cardinal advantage—it does not constipate. Assuming them equal in other respects, this alone is enough to stamp its superiority over bismuth, in the use of which we are constantly obliged to tease the gastro-intestinal surface with irritating purgatives. Another advantage not to be overlooked, especially in hospital practice, is this—the purified oxide of manganese can be procured at one-sixth the price of the preparations of bismuth. I possess notes of a great number of cases treated by manganese which incontestably prove its efficacy. Space will not at present allow of these details, but the result of its trial in forty hospital out-patients, men and women, briefly stated, must suffice. The duration of the disease ranged from three weeks (the shortest) to several months, and even years. The dose of the manganese was, in

almost every instance, ten grains taken three times a day before meals. Alterations in diet could have had little influence in these cases.

After having taken manganese one week,

Pain was quite removed	in 12	
Very much relieved	in 15	28
Relieved	in 10	
Unrelieved	in 3	
	40	

After having taken manganese two weeks,

Pain was quite removed	in 4
Very much relieved	in 15
Not so well as first week	in 1
Patients did not attend	8—28

Some of these patients continued taking the medicine for several weeks, and although a few who ceased to attend may possibly have remained unrelieved, no instance of failure came to my knowledge.

I was particular in ascertaining the effect on the bowels, and in four cases it is noted that the medicine was thought by the patients to constipate, in three of these only slightly. More extended observations, however, convince me that it has no constipating effect. In one case, on the other hand, it was stated to have purged.

Manganese is also highly useful in pyrosis, generally removing first the watery discharge, and afterwards the pain in a short time. In certain irritable states of the stomach it is also of much service. In one remarkable case, in which a woman had been affected with vomiting for many months, after the failure of bismuth, manganese was very successful in quieting the stomach.

The purified oxide of manganese may be given in doses of from five grains to half a drachm, according to the severity of the case. I tried the carbonate of manganese in a few cases with good results as regards pain, but in doses of ten grains it is apt to induce nausea, or even vomiting. The suphate given in solution also allays pain, but I found that even five grains is still more liable to disagree with the stomach. The ordinary black oxide of manganese is very impure, and therefore unfit for use. The purified manganese may be obtained from Messrs. Garden and Robbins, 372, Oxford Street, W. I shall feel indebted to any gentleman who may make a trial of the remedy, for a report of the results.—*Dublin Medical Press.—Glasgow Medical Journal, Jan.* 1865, *p.* 488.

23.—A CASE OF ILEUS ACCOMPANIED BY FÆCAL VOMITING SUCCESSFULLY TREATED BY GALVANISM DIRECTLY APPLIED TO THE MUCOUS MEMBRANE OF THE INTESTINE.

(Under the care of Dr. Stokes, Meath Hospital). Reported by Dr. J. M. Finny.

[The patient was a man of about fifty years of age. He had suffered from constipation, and took a dose of Epsom salts for its relief. The medicine acted next morning, but not so freely as usual, and he experienced pain in the abdomen about half an hour after its operation. This pain rapidly became very severe, and after a time vomiting and hiccup set in. The bowels were unacted on, the belly was swollen, hard, and tense, and injections failed to afford relief. On July 6th, ten days after the commencement of the illness the report is as follows :]

The patient is very restless, throwing his arms about, and frequently changing his position. He is covered by a clammy sweat; his face is drawn, and expressive of great anxiety; eyes sunken and heavy.

As all the treatment hitherto adopted has entirely failed in producing any decided and beneficial effect in evacuating the intestines, and as fæcal matters have been ejected by the mouth almost continuously for more than three days, ever since the night of 2nd, with the intermission of about fifteen hours on the 4th; it is evident in such a case that unless some more speedy and efficacious treatment be employed, the patient cannot be expected to exist much longer.

Under these most serious circumstances the application of the galvanic stimulus to the mucous surface of the intestine, was proposed by Dr. Stokes as a *dernier ressort*. The manner in which it was applied is as follows :—

The patient was directed to place himself on his hands and knees, and while in this position, with his buttocks well raised and his head low, I passed up the long tube with the intention of administering a large enema of ordinary house medicine, previous to the application of galvanism. The tube passed up readily as far as seven or eight inches, when it met some obstacle in its progress. On its withdrawal some feculent matter was observed at its end. It was again re-introduced, and by steady pressure and frequent injections, it slowly passed through some solid obstacle without at all conveying the idea of elasticity. Once through, it was readily passed up the intestine to its full length, and the remainder of the injection was thrown up. On removal of the syringe there was no escape of flatus or return of the injection. The exhausting syringe was now applied to the external extremity of the tube, but failed in causing any flow through it. Without withdrawing the tube the patient

was placed on his side, and one sponge, connected with the negative pole of the electro-galvanic battery, being applied to the margin of the anus, the other sponge, in connexion with the positive pole, was applied in a rapid manner over the whole external surface of the abdominal walls. After a few such applications there was a gush of fluid matter through the tube, which was then withdrawn, and the sponge which had hitherto been applied to the verge of the anus, was, along with the portion of the metallic handle to which it was attached, passed up the rectum two or three inches, while the same proceeding as before was renewed with the other sponge. Each sudden application threw the whole abdominal muscles into the greatest spasm, and was immediately succeeded by a gush of fluid fæces. Three large basinfuls were thus got rid of. The operation which had lasted about ten minuites, and which was attended with the most intense agony, so much so that the patient writhed under it to a painful degree, was now suspended, as great prostration of strength had ensued, the patient's pulse becoming almost imperceptible. He soon, however, rallied on the administration of brandy, and expressed himself greatly relieved. He then fell into a quiet sleep. In the afternoon he had two fluid stools, and eight more during the night entirely devoid of pain. No hard lumps were noticed to have been passed at any of these stools.

July 7th. The patient is greatly improved in appearance; he has had an easy and painless night's rest, and feels very much better; he has had no return of the hiccup or vomiting, and complains of no pain in the abdomen even when pressed upon; his appetite has already improved, and he wished for a chicken for dinner.

July 9th. Since the 7th he has rapidly recovered, his bowels up to to-day having acted regularly, and of themselves. The patient is, however, weak, and occasionally listless and heavy, as if his nervous system had sustained a severe shock.

Ordered a few compound rhubarb pills; oil to be taken at night.

July 11th. The patient left hospital to-day a little weak, but feeling otherwise perfectly well; his bowels are regular, and his stools natural.

On July 26th he again applied for admission, suffering from symptoms similar to those he presented before, but in a much less degree; his bowels having been confined for a few days, and complaining of some pain in the abdomen. He states that since he left hospital, on 11th, he has had two or three slight returns of abdominal pain, and that on one occasion his left leg swelled to a large size. The symptoms, however, left him after lying in bed for a few days. His present complaints are evi-

dently augmented by fear, as he is very nervous about himself, and dreads a repetition of his late treatment. This however, was not required, as, after a terebinthinate enema, a course of gentle purgatives, such as castor oil, administered every other morning, was sufficient to keep his bowels regular, and to free him of all pain.

This treatment was continued for about twelve days, and his strength and general appearance being much improved, he left hospital on 18th of August. Since then he has had no return of these attacks, and has been able to resume his work as a labourer.

In this case, so fraught with practical interest, there are some few points which particularly deserve attention.

In the first place, as to the immediate cause of the attack—what was it? In this is involved a question more easily asked than answered; for when no confirmation by dissection is afforded the actual cause must ever be clouded in a certain amount of obscurity, which no fancied diagnosis, lightened by the light of a far-seeing imagination, can ever thoroughly disperse.

The suddenness and severity of the attack following so soon after the dose of Epsom salts having been taken would at first almost point to the possibility of its being a case of poisoning.

Then, again, the fact of the pain being for the most part fixed in one locality, which in the first instance was relieved by the topical abstraction of blood, would be almost suggestive of enteritis; caused, perhaps, by an introsusception. Both these ideas are tenable for a time, but are not carried out by the after symptoms and progress of the case, as the pain both shifted its position, and though very intense the day preceding the application of galvanism, it had disappeared in four hours after the operation. To my own mind, taking into consideration the whole details of the case, is most strongly suggested the idea that the case was one of ileus, arising from a paralysed state of the muscular coat of portion of the great intestine. As all peristaltic action was absent the injections driven in by the force of the syringe were readily admitted past the affected portion of intestine; but were not again passed, from the loss of its normal muscularity. I should imagine that the violent peristaltic action produced by a large dose of sulphate magnesia, and the irritation of the mucous coat which it sometimes gives rise to, may have been the primary cause of, or in some way connected with, this loss of vital power in the muscular covering of the colon.

The other point of interest is the happy success with which this new mode of applying galvanism was crowned. It may safely be asserted that to its success the patient owes his life.

This, then, is a matter of the deepest importance, as it furnishes the physician with a line of treatment worth trying in those cases where the ordinary treatment, such as was adopted in the early part of this case, has failed, as it often will. The acute suffering which it gives rise to, should, however, be borne in mind, as it is not every case which would be sufficiently strong to endure it without receiving future detriment.

As it has been but once tried in the Meath Hospital, and I believe was never before made use of in the United Kingdom, it would be presumption confidently to promise the same success in all future cases where it may be applied; but from the extreme severity of the symptoms in the case above detailed, and from the speedy and effectual relief it gave, it is not too much to hope that its application in future cases may be followed by equally happy results.—*Dublin Quarterly Journal, Nov.* 1864, *p.* 341.

24.—ON THE SUCCESSFUL TREATMENT FOR COMMON TAPE-WORM.

By Dr. John Brunton, M.A., London.

[After considering the modes of introduction of tape-worm into the system, and the symptoms produced by it, Dr. Brunton relates the following case.]

I was called during the night to see Ed. G., a butcher, who was seized with an epileptic convulsion. I learned from his wife that he had often such convulsions, and that she thought they depended on an injury he had received to his head two years before. I prescribed the following:—

℞. Chloric. eth., ℥ss.; tk. hyoscyam., ℥iii.; spt. mindereri., ℥iii.; aq. camph. ad ℥vi. M. ℥j. 4tis horis sum.

Sinapisms to the nape of the neck, and perfect quiet.

He soon recovered, and was at his duties next day. I saw him in the forenoon, and on cross-examination found that he suffered from tape-worm. I then, after the usual preliminary treatment, prescribed ℥ii. of ext. eth. filic. mar. as a dose, to be followed by an ounce of castor oil. Next day thirty-five feet of worm were brought to me; but I still could not find the head, though it was apparent that nearly the whole worm came away, as the narrowest part of the worm produced was about the breadth of three plies of No. 6 cotton thread.

On tonic treatment E. G. remained well for four months.

Patient however, soon presented herself, as bad as before. I now resolved to give her kamela, which I had heard and read of as a first-rate anthelmintic. After preliminary treatment, and a good active purgative, I prescribed—

℞. Kamelæ, ℥iss.; mucilag., ʒi.; syrupi., q.s.; aq., ad ℥iii. M.

Half to be taken at bedtime, and half to be taken at 2 a.m.

The same result followed as with the male fern.

Patient got very well on tonics; but towards the close of 1862 she again presented herself, suffering all the symptoms as before indicated.

Finding that the male fern *alone* failed, that kamela *alone* failed, both in large single, double, and often repeated doses (for I prescribed kamela in repeated doses to another patient in the interval), I resolved to try a combination; I therefore prescribed the following (to be taken after the usual preliminary treatment):—

℞. Ext. eth. filic. maris, ℥iss.; pulveris kamelæ, ℥ii.; mucilag. et. syrup., q.s.; aquæ cinnamom ad ℥iii. M.

Half to be taken at bedtime, and half at 2 a.m.

Next day patient brought the *whole* worm, about twenty-six feet in length.

It was with great satisfaction that I viewed the result of my labours to expel the tænia. On examination, under a powerful glass, I found the head to bear the following characters:—Bulging in a pear-shaped form from the neck, it had four circular equidistant black discs on its extremity, having, in a certain light, a hollow appearance, and in another, a prominent aspect; in the centre a round orifice like a mouth, armed at the edge with a circle of hooks, evidently the prehensile part, by means of which the worm attached itself to the mucous membrane of the small intestine. Running from the edge of the black discs was a series of parallel canals, branching as they came down towards the neck, and connected together by numerous vessels. The canals had the appearance of being parts of the alimentary canal, if the black discs may be assumed to be feeding orifices, which I believe they are. The joints next the neck were very small and narrow—towards the tail, or end, they were large, about half an inch in breadth, and greater in length than breadth; each joint was marked alternately by a prominence on the centre of the edge, and a foramen connected with a duct and ovarium. "The ova are fertilized by a fluid that escapes from a vesicle which leads by a duct to the same opening as that from which the ovum escapes, so each segment is male and female in one." Hence I concluded this to be a Tænia solium.

This patient in February, 1865, is quite well, having had no return of the worm.

I have since treated ten cases with the last prescription, viz., a mixture of kamela and male fern, with complete success, on one trial; no second dose being required.

Two cases I may further relate as showing the results of other treatment.

Case.—E. T., butcher, had been under his master's medical attendant, a homœopath, for nine months, for the cure of tape-worm; came in July, 1863. I prescribed for him male fern and kamela, as indicated above. Next day he brought me four inches of worm, containing the head. Had tonic treatment since, and is now quite well and robust.

H. P., Esq., aged 28, had been at Buenos Ayres for two or three years, where he contracted tape-worm. He was treated there with turpentine, kousso, kamela, and afterwards, in various parts of Britain, with male fern and other drugs, with only palliative results.

When in London in May last year, while walking in the street, he was suddenly seized with severe spasm of the bowels. He was brought to me, and I prescribed the combined mixture with complete results.

In conclusion, I can most heartily recommend the use of kamela and male fern combined, as pretty certain in result. It would be bad logic to expect an universal and sure result from so few cases; but the result of my experience in the above cases, and the successful issue, after so many failures, leads me to conclude that the troublesome and disagreeable tænia can be very surely divorced from a connection that he has no right to have.

I may also remark that none of the patients had difficulty in retaining the medicine, though slight nausea sometimes followed its administration. Nor have any evil results followed from the large doses which were prescribed—purgation was the usual result, followed, sometimes during the act, at others after evacuation of the bowels, by the expulsion of the dead tænia. In none of the cases did the worm come away alive when male fern and kamela were given in combination; but in some of the failures before recorded, parts of the worm came out alive, and lived for some time after.

In none of the cases could more than one worm be detected. There are many cases on record in which two, or more, have been found.

I have prescribed as much as ʒiii. of eth. ext. of male fern at one dose, without success, further than palliation for a term of three or four months. In no case can we be certain of cure, unless the head of the worm be found. If it cannot (as it may escape notice in the evacuations), and a period of at least six months has elapsed after evacuation of the worm without return of the symptoms, then the cure may be considered complete.—*Glasgow Medical Journal, April* 1865, *p.* 52.

DISEASES OF THE URINARY ORGANS.

25.—ON THE CONDITION OF THE KIDNEY IN THE GOUTY.

By M.M. Charcot et V. Cornil.

Two forms of lesion of the kidney are found in gouty subjects. The first although dependent on gout, does not present any feature distinctive of it, whilst the other especially belongs to it. In the first category the lesions are simply those of albuminous nephritis; the second comprises deposits of uric acid or of urate of soda in the kidney. These renal deposits present two defined forms accordingly as they are formed of uric acid or urates. In well-marked cases, the urates deposited in the kidney in white lines are as characteristic of the disease as are the concretions of the same kind in the articulations. The authors, whilst they allow the frequency of the occurrence of the small, hard, atrophied kidney, in which the cortical substance is diminished—first described by Todd as "the gouty kidney"—in the gouty, deny that it is peculiar to the disease. They say that the same condition of the organ may be found in other diseases besides gout; and on the other hand they assert, on the evidence of cases observed by Garrod and by themselves, that the kidneys of the gouty may present anatomically all the lesions of Bright's disease (albuminous, desquamative, or parenchymatous nephritis). They give Garrod the credit of having first accurately described the special form of deposits of urate of soda in the kidneys. These appear as linear white deposits, following the direction of the tubules of the pyramids; the deposit shows a crystalline structure consisting of prisms of urate of soda. Their composition is always the same, as shown by the production of murexide on the addition of nitric acid and ammonia, and by crystallization of uric acid on the addition of an acid. The question, whether the deposit of the urate of soda is situated within or outside the uriniferous tubules, is an open one. Garrod's first observation led him to think that the white lines were due to a filling of the uriniferous tubules; and this also was Mr. Ceeley's conclusion, in a case communicated by him. Subsequent observations, however, led Dr. Garrod to modify his opinion. The crystals of urate of soda, commonly larger in the kidney than in the cartilages, seemed frequently to be situated in the cellular tissue rather than in the cavities of the tubuli. In a case observed by the authors, at first sight the crystals seemed placed in the intervals between the tubuli; but by adding acetic acid and dissolving the free crystals, it became clear that a part of the deposit was seated in the interior of the uriniferous tubules. The following are the authors' conclusions:—

1. That in a certain number of the gouty, in consequence of the irritation produced in the kidneys by the passage of a of a large quantity of urates, albumen appears in the urine, irregularly and in small quantity; and this condition is sometimes accompanied by œdema.

2. That these symptoms correspond with an anatomical alteration of the kidneys, which consists in a chronic form of albuminous nephritis (parenchymatous nephritis), or in a chronic alteration, characterized by atrophy of the parenchyma, with thickening of the fibrous partitions and of the arteral coats (interstitial nephritis, *gouty kidney* of Todd,) but that these lesions have nothing which pertains specially to gout.

3. That two forms of deposit in the kidney belong properly to gout—1. Deposits of uric acid (yellow or red granules), the *néphrite goutteuse* of M. Royer; 2. Deposits of urates, which are characteristic of gout, and completely identical with those of the articulations.—*Brit. and Foreign Medico-Chirurgical Review, Jan.* 1865, *p.* 257.

26.—ON GOUT.

By Dr. H. Bence Jones, A.M., F.R.S., &c.

The gouty diathesis consists in an excess of urate of soda not only in the serum of the blood, but in the fluid that diffuses from it all the vascular and non-vascular textures of the body. An attack of gout is a chemical process of oxidation set up in the parts where the urates are most able or liable to accumulate. By the oxidising action the urates are wholly or partly changed into urea and carbonates, which can more readily pass from the textures into the blood, and be excreted by the kidneys, skin, and lungs. The oxidation even in the bloodless textures causes increased flow of blood and mechanical pressure in the vessels nearest to the inflamed part, and hence pain and redness, and then swelling and œdema, proceed. Though the gouty diathesis is a disease of the textures as well as of the blood, yet in its origin and situation an attack of gout is even more a disease of the tissues than a disease of the blood.

The urate of soda bears the same relation to gout that sugar does to diabetes; and as the want of oxidation of sugar is the cause of the diabetic diathesis, so the want of oxidation of the urates, and their consequent accumulation in the textures and blood, is the cause of the gouty diathesis. Errors excepted, as almost every grain of starch passes through the stage of sugar, so almost every grain of albuminous substance that enters the blood sooner or later, in its way out, passes through the stage of uric acid, and, if thoroughly oxidised, escapes as urea, carbonic acid,

and water. The number of substances that are formed between albumen and urea are vastly more than between starch and carbonic acid; but whatever their number, uric acid is the penultimate, and urea the ultimate, product of oxidation. Hence there are at least two ways in which an excess of uric acid may occur in the blood and textures : 1st from an excess of animal or vegetable albuminous food entering the system,—*i.e.*, from excessive production; and 2nd, from an arrest of oxidation,—*i.e.*, from want of destruction. Of course, the greatest accumulation of uric acid will occur when the albuminous food is excessive, and when at the same time the oxidation is least. Even if no excess of albuminous food is taken, yet if the oxidising action is deficient, uric acid may accumulate in the serum, and it will immediately diffuse even into the bloodless textures. On the other hand, an excess of albuminous food may be taken, and, provided the oxidising action is also in excess, no accumulation of uric acid in the blood or textures may occur. It is therefore evident that there are two modes of preventing the gouty diathesis; 1st, by diminishing the amount of vegetable and animal albuminous food; and 2nd, by promoting oxidation. In other words, the smallest amount of food and the greatest amount of air are the antidotes for the gouty diathesis. If an excess of fresh air is taken whilst a large quantity of food is eaten, these cause no gouty diathesis as long as the antidote (oxygen) destroys the *materies morbi* (urates); but if from any cause the oxidation becomes less, the urates accumulate in the liquor sanguinis, and pass by diffusion into and around the cells of all the vascular and non-vascular structures of the body, and remain unoxidised where there is least oxidation, and even form crystals of urate of soda in consequence of the slow deposition of the urates on the surface and within the structure of the non-vascular cartilages. Where urate of soda accumulates it acts like a foreign body, increasing friction, setting up irritation and inflammation, by which the uric acid is more or less oxidised, and thus being made soluble, it is more or less removed, and when the inflammation subsides, effused fibrin almost alone may remain to mark the situation of the attack.

If no inflammation comes on, the liquor sanguinis may free itself of urates by the kidneys, and then the deposited crystals may more or less entirely re-dissolve, and diffuse back into the blood; or, if the serum continues full of urates, the chalk stones may gradually increase in the direction of least resistance, making the joints more and more rigid, and the skin more and more thin, until the pressure breaks the surface, and the chalk stones escape as myriads of microscopic crystals. Thus, the fingers and toes may become supplementary kidneys, to the

great relief of the blood and textures, and the gouty diathesis may almost be considered as microscopic gravel in the textures, and an attack of gout as a chemical operation for the removal of the gravel from the part in which it had accumulated.

That the urates diffuse into the synovial fluid and on to the surface of the cartilages, the following case shows :—W. W , aged 48, was admitted into St. George's Hospital on November, 23, 1855. He was a painter and glazier in good circumstances, and had suffered more or less from gout for eighteen years ; otherwise he enjoyed good health, and never had colic. The urine contained a small quantity of albumen ; the heart's action was feeble ; the right hand was red and swollen ; his feet were slightly œdematous ; chalk stones existed in his ears. The acute attack of gout in a fortnight gradually subsided, and he was about to leave the house when a fresh inflammation of the hands, feet, and knees came on, with great prostration of strength. In a few days he was seized with epileptic fits ; the slightest motion immediately brought on an attack ; the pupils were contracted, there was excessive tremor of the limbs, and he sank four weeks after his admission.

Both knee-joints were carefully opened. They contained two or three drachms of thick synovia, and in each cavity there was a flake of fibrin about an inch and a-half long, shreddy, with whitish specks visible to the naked eye. On examination under the miscroscope, small acicular crystals were seen mixed with fibrinous matter, and when a drop of dilute hydrochloric or acetic acid was added, the whole surface of the fibrin in a few minutes became covered with microscopic uric acid crystals. The moist fibrin from both joints showed these crystals of uric acid, and when the fibrin was treated with nitric acid and evaporated, there was distinct evidence of uric acid. On the surface of the cartilage there was only a very small spot free from the deposit of urate of soda. Both great toes at their tarso metatarsal articulations contained the same deposit on the surface.

The spleen was large and soft ; the kidneys granular, and the cortex much wasted. There were some small deposits of red sand visible to the naked eye in the mammary processes.

A man, aged forty-one, was admitted into St. George's Hospital with continued fever, and died on August 20, 1849. On examination by Dr. Handfield Jones, gouty deposit was found in both knee-joints ; 1st, under the synovial membrane at the margin of the cartilage ; 2nd, in the superficial structure, or on the surface of the cartilage, from whence it could not be removed by gentle washing or scraping ; 3rd, in the substance of the cartilage at some depth ; 4th, in the cancelli of the subjacent bone (the patella).

The gouty matter appeared for the most part as aggregations of amorphous masses, varying in size, and encrusted over frequently with minute crystalline spiculæ.

The cartilage was remarkably thickened, being at least three times its ordinary dimensions. Near its middle it was much more elevated than towards its margin, and very soft and yielding. When a vertical section was made of it, the surface exhibited a marked fibrous structure, quite visible to the naked eye, the fibres being arranged vertically to the surface, and being much more marked in the deeper than in the superficial layers.

The microscope showed that the basis substance was considerably increased, the cells being in much scantier proportion than natural, but not essentially altered. The fibres were not isolated from each other. The fibres or bands were of some considerable width, separated by narrow intervals of healthy structure.—(*Proceedings of Pathological Society*, vol. ii., p. 103.)

On the Means of Detecting Gout.—The chemical test for the gouty diathesis—that is, for excess of urates in the blood and textures—is based on an analysis of the urine and of the serum of the blood.

Even long before the gouty diathesis is formed, the urine will give warning of the future disease. An excess of urea in the urine may be caused either by an excess of exercise or by an excess of vegetable or animal albuminous food. The amount of urea bears a certain, though not direct, proportion to the amount of daily exercise; when the exercise is moderate, an excess of urea in the urine indicates that albuminous food in excess is passing through the system; when the oxidation of the albumen is perfect, urea and carbonic acid are formed; but with less perfect oxidation, urea and oxalic acid result. Hence, oxalate of lime, with excess of urea, constitute the earliest evidence of imperfect oxidation of an excess of albuminous food. When the oxidation is still less, then oxalic acid and urea are not formed from the uric acid, but the urine shows an increase in the quantity of urates.

Simple inspection of the urine is quite worthless as a means of determining the amount of urates in the urine, for the clearest urine may have the greatest amount of urates dissolved in it, and urine that thickens on cooling may contain less uric acid than perfectly clear water. You will see in a paper I published in the Philosophical Transactions, part ii., 1849, p. 249, that urine passed at 5.10 p.m. became thick on standing, and contained 0·52 grain uric acid per 1000 grs. urine; urine passed at 11 p.m. remained clear on standing, and contained 0·87 grain uric acid per 1000 grs. urine. Again (p. 251), urine passed at 7·35 p.m, became thickish on standing, and contained 0·29 grain

uric acid per 1000 grs. urine; urine passed at 10·5 p.m. remained clear on standing, and contained 0·33 grain uric acid per 1000 grs. urine. Again, urine passed at 7·55 p.m. became thickish on standing from urates, and contained 0·31 grain uric acid per 1000 grs. urine; urine passed at 10·45 p.m. remained clear on standing, and contained 0·20 grain uric acid per 1000 grs. urine.

When analysis shows that an excess of uric acid is thrown out of the body, this is a proof that the gouty diathesis is ready to form whenever the kidneys cease to remove the excess from the system. The presence of any urates at all in the urine is a sign that oxidation is not so perfect as it might be, and the more urates in the urine the more imperfect the oxidation in the system must be considered; but as long as the products of the imperfect oxidation are thrown out by the kidneys or skin no gouty diathesis is produced.

Thus, then, first, excess of urea when little exercise is taken; secondly, oxalate of lime with excess of urea; and thirdly, urates in excess, by analysis, constitute the three tests of the different degrees of suboxidation which precede the establishment of the gouty diathesis.

When uric acid in excess is in the system, analysis of the urine may not indicate any excess there—indeed, the uric acid may be be absent from the urine and be retained in the blood—as, for example, in Bright's disease. Then the chemical examination of the serum may give positive proof of the existence of this imperfect oxidation, and lead to the certainty of the textures containing the same urates ready to set up inflammation of a gouty nature at any moment; usually symptoms of gout begin to make themselves felt by pain, followed by heat and redness, and then the chemical methods for detecting gout are usually quite unnecessary.—*Medical Times and Gazette, April* 22, 1865, *p.* 407.

27.—ON ACIDITY, OR THE ACID DIATHESIS.

By Dr. H. Bence Jones, A.M., F.R.S., &c.

Acid and sugar, chemically, are as closely related as parent and child; and hence a great similarity exists between the symptoms that arise from an excess of sugar and an excess of acid. The nature of the acid diseases, the symptoms it produces, the means of detecting it, the consequences it occasions, and the treatment requisite to cure it, are all essentially chemical questions. A most important difference, however, exists between the sweet and sour disease, namely this: the sweet disease is mostly free from secondary mechanical diseases; whilst acidity continually gives rise to mechanical complaints. Two of these

secondary mechanical, or mechanico-chemical, complaints—gravel and gout—are the frequent lot of those who, living more by nerve than by muscle work, disarrange the healthy balance between the carbon and oxygen which go in and come out of the body.

Acid is essential for the performance of the functions of the body. Without acid the albuminous food could not be dissolved; the excretions from the blood could not be safely removed. If no acid was produced in the body, there would be no digestion and no depuration of the blood, and probably bony matter would accumulate everywhere. The vinegar cruet is only less essential to man than the salt cellar, because the animal machine is itself an acid factory. Very often more acid is made than is useful or beneficial; then chemical errors occur, and these set up mechanical diseases as surely as the effect of the cannon-ball depends on the combustion of the gunpowder. The mechanical results are vastly more serious than the original chemical action; but they are directly proportioned the one to the other. As the motion of the mass depends entirely on the motion of the molecules, so the mechanical disease arises solely from the chemical disorder.

It would be useless, even if it were possible, to give a list of the mineral, vegetable, and animal acids, between carbonic acid (CO_2) and uric acid ($C_{10}H_4N_4O_6$), which may be formed from changes in the textures of the body, or from the food. It is far more important to consider how an excess of acid may arise or accumulate in the system. This may occur in two ways—1st, by over-production; and, 2nd, by insufficient evacuation. When these causes co-operate the excess of acidity reaches its maximum.

First, on the production of acid in the body from the textures and food.

The living human body is, chemically, a varying mass of more or less complex carbonaceous matter, each portion being exposed to the action of oxygen. The chemical products everywhere vary according to the substances acted upon, and according to the time during which the oxidation is continued.

Outside and inside the capillaries, in every part of the body, in cells and out of cells, chemical changes are taking place. The oxygen of the air, the organic fluid, the catalytic membrane, act and re-act each on the other; and the products of the chemical action are reabsorbed into the blood, or are thrown upon the inner or outer surface, to be taken up by the alimentary mucous membrane, or to be removed by the lungs, the skin, or the kidneys.

At present, the chemical changes which take place in the textures are very imperfectly known, and the variations of

action which occur in health and disease are only beginning to be observed. The healthy muscle, during the circulation of the blood through it, has no acid reaction; but for a time after death chemical actions continue, and sufficient acid is produced, and remains unneutralised by fresh alkaline liquor sanguinis, so that markedly acid reaction can be obtained. Even during life, in tetanus, sufficient acid may be formed to give an acid reaction, and after violent epileptic convulsions excess of acidity may be found in the urine. Even the nerves and the electric organs of fishes show signs of acidity when the alkaline blood ceases to flow.

The production of some healthy and less complex secretion may enable us, from its chemistry, to obtain greater clearness on the production of acid in the body. The gastric juice furnishes by far the most striking example of the formation of acid in the body. Until the chemical acid factory in the stomach is fully understood, the chemical changes which are going on outside the capillary vessels throughout the body cannot be realised.

There can be no doubt that the formation of gastric juice occurs outside the capillaries of the stomach. Its rate of secretion depends on the rate of passage of the blood through the capillaries; and this is directly under the control of nerves that open or shut the blood vessels. In the fluid poured out of the capillaries chemical changes occur, acids are formed—perhaps Dr. Marcet's colloid acid; or by the oxidation of sugar, among other acids, oxalic acid may be produced, and this may decompose the chloride of sodium, and the most diffusible hydrochloric acid may be poured out on the free surface of the tubes; whilst the alkali passes with the organic acid, or its products of combustion, back into the blood.

In the Medico-Chirurgical Transactions, vol. 35, p. 41, you will find a paper "On the Alkalescence of the Urine from Fixed Alkali in Some Cases of Diseased Stomach." I shall here give you only one example in order to fix this inverse relationship of the acidity of the stomach and urine firmly in your minds. A man, aged forty-six, was admitted into St. George's Hospital with sarcinæ ventriculi. The eruption from the stomach occurred at irregular periods, once and sometimes twice daily; the quantity vomited each time was from one to four pints of intensely acid fluid. The urine was usually passed about six times in twenty-four hours, when the vomiting was most The urine was acid twice, or at most thrice, daily before the sickness occurred; and for the rest of the day after the vomiting the urine was strongly alkaline from fixed alkali. The patient remained in the house for a month, and during the last week he had only one attack of sickness; then the urine became

markedly acid each time it was passed, and remained so throughout all the day.

After digestion is finished, when the food is dissolved in the stomach, the intensely acid fluid is in part returned into the blood. The serum might almost cease to be alkaline but for the neutralising action of the bile; thus the degree of alkalescence of the blood must be continually changing, and the fluid effused from the capillaries, and out of which the secretions are formed must become less alkaline after the food is absorbed than it is whilst digestion is going on in the stomach.

An oxidation similar to that which occurs in and around the stomach cells must occur to a greater or less degree everywhere throughout the body; carbonic acid or other acids, arising from a less perfect combustion, must be formed outside the capillaries of the body. In the muscles and nerves, and in every other texture, power must be set free by the mutual action of the carbon and the oxygen, and the acid products would quickly neutralise the alkalescence of the blood if the acids did not pass off by the lungs, the skin, and the kidneys.

On the Removal of Acid from the Body.—Without cessation night and day free acid continues to pass into the air vesicles of the lungs from the blood, thus rendering it more alkaline than it would otherwise be, and counter-balancing not only the acid produced by oxidation in the different textures, but also the residue of the acid from the gastric juice and food which is not neutralised by the bile.

If the combustion in the body were complete, the whole of the products, except the ashes, might escape by the lungs; free respiration might carry of readily all the acid produced in the system, but imperfect oxidations do occur, and then other acids besides carbonic acid are formed and give rise to neutral or to acid salts which are not volatile, and these being unable to pass off by the lungs are thrown out by the skin and the kidneys.

There can be no doubt whatever that acid is produced outside the capillaries of the skin and kidneys in the same way, although to a much less degree than in the stomach cells. The chemical composition of the substances that pass into the urine and the sweat depends not only on the amount of action of the oxygen, but also on the chemical composition of the fluid poured out of the capillaries to form the secretion.

If the serum of the blood is at its highest point of alkalescence, the acids formed by oxidation during the secretion of urine may be too feeble to neutralise the alkali poured out, and alkalescent urine may result. If, on the contrary, feebly alkaline or acid salts are effused from the capillaries, a very small amount of additional acid produced by oxidation in the secreting struc-

ture of the kidneys will set free some of the acid from the acid salts, and thus free uric acid may appear in the urine.

The same formation of acid occurs in the secreting structure of the skin, and although it is not possible to demonstrate the daily variations of the acidity of the sweat, yet the extraordinary increase in its acidity with strong exercise, or in some diseases, as in rheumatic fever, shows that it is subject to great variations.

The variations in the alkalescence of the saliva were long since shown by Dr. Wright to be influenced by the digestion.—(*Lancet*, 1841, p. 787.)

The smallest amount of acid is thrown out of the system when the respiration is impeded, whether from insufficient exercise or from imperfect supply of air; then carbonic acid accumulates, and the less perfect products of combustion are increased in the serum. If these can escape by the kidneys and skin the system is relieved, and no secondary symptoms are produced; but if these vents are closed, and if by acid diet and an irritable stomach an excess of acid is poured into the blood, then an outburst must take place, and chemical and mechanical diseases—that is, unusual molecular or massive motions—occur until the healthy condition of the blood is again reached.

Excess of acidity or the acid disease may shortly be defined to be a want of free oxidation and an insufficient escape of the products of imperfect combustion.

On the Means of Detecting Acidity.—It is not easy for the Physician to determine accurately the escape of an excess of acid by the lungs, and still less so by the skin; but by both chemical and microscopical evidence he can follow the minutest changes which occur in the acidity of the urine, and thus he may know something not only of the state of the blood from which that urine is secreted, but he may obtain some information regarding the acidity of the processes of oxidation and nutrition outside the capillaries. Different litmus papers vary so much in composition and delicacy of reaction that no satisfactory conclusion can be reached by this test alone. A standard solution of soda furnishes the surest chemical proof of the amount of acid in a given quantity of urine, and thus variations may be proved and comparisons made with the utmost accuracy. But a readier method of coming to a practical conclusion exists in observing by the microscope the state in which the uric acid exists in the urine. The delicacy of this test for free acid far surpasses any other: indeed, litmus paper and standard solution cannot enable you to distinguish between acid salt and free acid; but the microscope by showing more or less free uric acid crystals tells at once the greater or smaller amount of free acid in the urine. As long as free uric acid crystals do not exist in the urine no

very great amount of acidity is present. When acid urates alone are passed there may be very great effect on the litmus; but as long as the microscope and chemistry show that free uric acid is absent, the chief secondary mechanical disease, uric acid stone, is not likely to be set up.

Hence the determination of the presence or absence of free uric acid in the urine is a question of great importance.

A small bottle or test tube should be filled with fresh urine and examined by transmitted light. Uric acid crystals can be easily seen. If no red sand is present in twelve hours another examination should be made. Very frequently a deposit occurs as the urine cools, then the microscope must be used to see whether crystals of uric acid are mixed with the granular deposit. The amount of free acid may be known by the rapidity with which the uric acid crystals appear, by the size of the crystals, and by the more or less perfect decomposition of the granular urates.

When the free acid is in very great excess the uric acid may crystallise in the renal tubes or in the pelvis of the kidney or in the bladder. When very little free acid and very little uric acid has been present, I have seen the crystals of uric acid appear on the eleventh day after the water was passed.

Professor Scherer long since stated that an acid fermentation took place in the urine soon after it was passed; but I have been unable to satisfy myself that any increase of acidity occurs. His experiments were not made on healthy urine. When he made them, the state in which the uric acid existed in the urine was not known. The slow deposition of uric acid from a feebly acid solution of urates was not taken into account.

Very frequently, when examining the same specimen of urine day after day for uric acid crystals, I found that no free acid was deposited; until at last I was led to make exacter experiments to determine whether the free acid in the urine was increased after the water was made. But first a series of twenty-seven experiments were made on healthy urine to determine how often, and how soon on standing, uric acid crystallised out. Four times only were these crystals found by the microscope twenty-four hours after the urine was made; and five times the acid crystallised out in two days. Eighteen times no uric acid was found free. In other words, eighteen times out of twenty-seven the urine under the microscope did not show this evidence of an increase of acidity. In the other nine times the uric acid might possibly have been set free by an acid fermentation, although another explanation was probable, namely, that the crystals only formed slowly because no great quantity of urates were present, and because the liberating acid was very weak. For determining between these views, a standard alkaline solu-

tion of caustic soda was prepared, and the acidity of the urine was measured as soon as possible after the urine was made, and then from time to time afterwards the acidity of the same urine was again noted.

When it was proved that sugar existed in small quantities in healthy urine, I thought that some evidence for or against an acid fermentation might be got by determining the amount of sugar in the urine, first when fresh, and afterwards when it had stood some days.

Three litres of healthy urine were divided into two equal parts. The one half was examined for sugar immediately; and the amount of sugar was 2·4 grains. The other half was left for twenty-five days, until it became feebly alkaline, and then the sugar was determined, and the amount was 1·9 grain. A loss within the limits of error in the method of analysis.

These and many similar experiments are opposed to any acid fermentation; and when the formation of uric acid crystals in the urine occurs, it may be taken as a proof that some free acid is secreted by the kidneys, and that this, sooner or later, has set free more or less uric acid from the acid urates in the urine.

The longest time in which I have seen uric acid crystallise out has been eleven days, but usually the first, second, or third day after the water is made the crystals form, if they form at all; and the formation of these crystals is the only certain and delicate test of the presence of free acid in the urine. I had hoped that the form of the crystals, varying as it does so greatly, might enable me to determine the nature of the liberating acid; but after a long series of experiments I can as yet come to no satisfactory conclusion in this respect.

Symptoms of Excess of Acidity.—Only when the skin or mucous membrane have become irritable by the continued action of the acid, do symptoms of acidity force themselves on the notice and require Medical aid. The amount of uneasiness or pain will vary with the sensitiveness of the membrane and with the degree of acidity. Slight degrees of acidity hardly irritate the skin; but itching, nettle-rash, eczema, and herpes are the outbursts of an over-acid state.

The mucous membrane of the stomach is less sensitive than that of the pharynx, œsophagus, and bowels. When 162 grs. or even 81 grs. of dry tartaric acid in four or six ounces of water are taken, the acid hurts the mouth and the upper part of the food-tube, and then ceases to be felt for three hours or more, when it causes violent griping pain for an hour or two. When the mucous membrane has become irritable the stomach becomes much more sensitive, and acidity causes pain, cramp of the muscular coat, and vomiting; the bowels feel more and

move more strongly, and violent pain, colic, tormina, and tenesmus come on.

If the sensitiveness of the mucous membrane of the urinary organs is increased, the nerves and muscles become irritable; urgency and frequency of passing water gradually increase into constant pain and violent spasm of the bladder. In the urethra a scalding pain is felt, and constriction of the mucous membrane causes a complete stricture whilst the spasm lasts.

When the acidity is excessive, the skin, stomach, and urinary organs may all be simultaneously irritated. In other words, the amount of acid formed may be so great that it is thrown out everywhere. Most frequently the acidity only shows itself in the stomach, and in the urine; and the symptoms produced in the stomach and urinary organs are so closely dependent that the same treatment that does good to the one mucous membrane will be found to do good at the same time to the other.

I might here, if time permitted, give you innumerable cases of acidity illustrating the great persistence and intractability of the deposit of uric acid, and its great liability to return as soon as the alkalies are omitted, unless the strictest diet is observed. The resemblance of these cases to cases of diabetes is very striking; and, moreover, in slight diabetes or intermittent diabetes the production of excess of acid is very frequently to be met with.

I must, however, give you two cases illustrating the rarer consequences of this excess of acidity. A gentleman, aged forty, consulted me for constant deposit of urates and uric acid in the urine. He had been subject to these deposits for years; for the last few months occasionally, from one to five hours after a late dinner, he was attacked by pain in the stomach, which in a short time became intermittingly spasmodic. The intensest pain was reached in half a minute, it then relaxed, and returned as badly as before in two minutes. His suffering lasted about an hour, when the pain gradually abated, leaving a tenderness on pressure and an irritability after food for two or three days. The urine passed after the attack gave on standing uric acid crystals. I advised him when another attack occurred to fill the stomach with hot water and then to reject it. This he did, and a considerable quantity of nearly clear acid fluid was thrown up; on repeating the emetic later during the same attack, a much more intensely acid fluid was obtained, showing that probably an hour-glass contraction of the stomach had existed. By careful diet and anti-acid medicines the attacks entirely ceased. In another patient, six or eight hours after food, attacks of violent cramp used to occur in the rectum, and last from half-an-hour to an hour.

Relief at the time was obtained by pressure on a hard seat, and by careful diet and alkalies the attacks entirely ceased.

Before speaking of the treatment, a few words must be said on the

Consequences of Excessive Acidity.—In all I have hitherto said of diabetes and acidity a parallel might be drawn between the two diseases, with this difference—that the bland sugar acts as a diuretic, whilst the irritating acid affects the nerves and muscles, causing pain and spasm; but in the consequences of the two diseases an entire divergence must be noted. Diabetes causes a painless weakness and atrophy. Acidity is the cause of from 60 to 50 per cent. of all the cases of stone that occur in England. Dr. Prout says—"If a uric acid nucleus had not been formed and detained in the bladder, two persons, at least, out of three who suffer from calculus would never have been troubled with that affection."—(p. 583 "Stomach and Renal Disease.")

In the 26th vol. of the Medico-Chirurgical *Transactions* for 1843, I have given the analysis of the calculi in St. George's Hospital. In 233 calculi, 89 consisted of uric acid alone; that is, 38 per cent. of these stones were caused *solely* by acidity. Whilst 135 consisted of uric acid; uric acid and oxalate of lime together; and of these substances mixed with urates; that is, 58 per cent. were wholly or *partially* caused by acidity.

Gravel is still more frequently caused by over acidity. Seventy-five per cent. of all renal calculi are uric acid, caused by acidity alone. Hence three out of four renal attacks giving rise to almost as violent mechanical suffering as the human body can endure, are caused by a slight chemical disorder which often continues unobserved until the mechanical disease is set up.

Treatment of Acidity.—The relation that exists between diabetes and over acidity is even more evident in the treatment than in the causes and symptoms of these diseases. The best diet for diabetes is the best diet for over acidity. The same medicines are most useful in both complaints. The same immediate effect can be produced by treatment, and there is the same disposition to a relapse after the symptoms have entirely ceased when wrong food is taken.

The treatment of acidity must be considered under two heads: First, the diet; and secondly, the medicines.

With regard to the diet. All that has been said against saccharine and farinaceous diet in diabetes might be repeated here, inasmuch as these substances give rise in the system to various acids in their progress to carbonic acid and water. Hence, as regards the action of sugar, dextrine, and starch, I may refer to the lecture on diabetes; but as the acid diathesis

arises from a slighter check to the chemical actions of the body than the saccharine diathesis, therefore a less strict diet will give a greater improvement in this disease than in diabetes. Moreover, medicines effect a more decided good in over acidity than in the saccharine complaint, and on this account, also greater liberty may be given in diet provided the acidity from the food is neutralised by alkaline medicine.

Another difference must be made in the diet of those who suffer from these complaints. In diabetes, fatty substances, cream, butter, fat, and oil check the wasting of the body; but in excess of acidity these articles of food often give rise to great uneasiness in the stomach; they are very liable to cause a peculiar feeling of irritation, which is known as heartburn. Thus, over acidity and heartburn often can be removed and kept away by leaving of sugar and butter. In severer cases, potatoes, rice, arrowroot, and a considerable portion of the daily bread must be given up. Fruits of all kinds are mostly highly flavoured ether mixtures of starch, sugar, and acid. Wines and beer chemically differ from fruit only by containing alcohol instead of some starch and sugar. Alcohol itself, very probably in the stomach as well as in the air, gives rise to acid, and the weak spirit and sugar of half-fermented beer and effervescing wine more easily undergoes this acid fermentation than fully fermented wine and pure spirit. Hence, in extreme cases of acidity small quantities of lean animal food, including fish, flesh, fowl, game, and eggs, and brandy-and-water, constitute the best anti-acid diet. All fluids containing sugar should be avoided; and alkaline gaseous mineral water, natural or artificial, will neutralise a portion of the acidity which the imperfect combustion continually produces.

Exactly the opposite diet was advised at the dawn of animal chemistry, on the ground that the ash of vegetables containing alkalies, these would neutralise all the acid that should arise from the starch and sugar. But take, for example, potatoes: the amount of acid producing substances is from 20 to 30 per cent; the amount of alkali at most will not reach to 1 per cent. Or take bread: the ash of bread would neutralise at most 1 per cent. of acid. The amount of acid-producing substances in bread is 46 per cent. Any other fruit or vegetable would give equally good evidence against a vegetable diet in over-acidity.

The object of medicines in this complaint may be divided into —first, the removing, and secondly, the neutralising of acid.

The removal of acid is effected most immediately and directly by emetics, emptying the stomach at the time when the greatest quantity of acid is present, that is, during the digestion of a full meal. In the treatment of the acidity of children, this is the shortest and readiest way. Hot water, mustard and water,

ipecacuan will remove more acid than would be neutralised by the usual amount of alkali that could be taken daily. The repetition of the emetic should depend on the re-appearance of uric acid crystals in the urine.

The free action of the skin by baths, especially Turkish baths and vapour baths, or by strong exercise, removes acid quite as effectually, although more slowly, than by taking away the gastric juice. Moreover, an increased natural action disarranges the system less than a stomach catastrophe, which, though scarcely felt by the young, is a shock unfitted for advancing years. Of all the methods of exciting perspiration strong exercise is the best, because this generally implies a freer respiration, and this means the greatest possible conversion of acids into carbonic acid and water. The more bracing the air is, and the more free from mechanical and chemical impurities in the form of dust, smoke of all kinds, fogs, and noxious gases, the more vivid is the combustion, and the greater is the amount of acid that is removed from the system through the lungs.

There remains to be considered the removal of acid by the kidneys. By increasing the action of the kidneys, an increased quantity of acids of many kinds, from carbonic acid to uric acid, is thrown out of the system. Diuretics thus become veritable anti-acids. Hence, pure water and nitre and mineral waters, by exciting diuresis, relieve the blood and textures of acid, but whilst promoting the action of the kidneys the acid may be neutralised, and this is the second object to be obtained by medicine.

In neutralising the acidity of the urine, by far the most effectual medicines are vegetable salines, containing fixed alkalies or earths.

Although five drachms of tartrate of ammonia be taken in the day urine is not made alkaline, nor even neutral; whilst after the same quantity of tartrate of potass the urine is alkaline for the greater part of the day, and at one period of the day reaches as low as thirty-two degrees; in other words, a thousand grains of urine contained three grains of carbonate of soda. Moreover, eighty grains of carbonate of ammonia daily in three different experiments did not make the urine neutral, though it acts more strongly than most alkalies in neutralising the acidity of the gastric juice in the stomach. Three drachms and a-half of liquor potassæ, containing $6\frac{1}{4}$ per cent. of potass, taken in one day, though it lessened the acidity, did not cause alkalescence; still, no doubt, the action of this and other alkaline remedies, when long continued, produces very decided effects in neutralising acidity, the comparative worth of equal quantities of different alkalies may be determined by their combining proportions.

Thus 10 parts lithia		= 12	parts	ammonia.
,,	,,	= 14	,,	magnesia.
,,	,,	= 19	,,	lime.
,,	,,	= 21	,,	soda.
,,	,,	= 33	,,	potass.
,,	,,	= 54	,,	bismuth.
,,	,,	= 25	,,	carb. of lithia.
,,	,,	= 27	,,	carb. of ammonia.
,,	,,	= 29	,,	carb. of magnesia.
,,	,,	= 35	,,	carb. of lime.
,,	,,	= 37	,,	carb. of soda.
,,	,,	= 48	,,	carb. of potass.
,,	,,	= 70	,,	carb. of bismuth.

In other words, 14 grains of magnesia will neutralise as much acid as twenty-five grains of carbonate of lithia, or forty-eight grains of carbonate of potass, or seventy grains of carbonate of bismuth. One grain of carbonate of lithia is nearly equal to a grain and a half of carbonate of soda or two grains of carbonate of potass.

In ordering these different alkaline substances, their constipating or their aperient action must be considered; and beyond all this, the peculiarities of different individuals must be taken, provided it does not cause pain or blood. The amount be learnt by experience. That carbonate of soda to the amount of two ounces and a half daily may be taken for months with impunity may be seen in a case in Med.-Chirurgical Transactions, vol. v., p. 80. The blood had a strong buffy coat. The urine was alkaline. In speaking of the treatment of diabetes, I have dwelt on the alkaline action of different mineral waters. I must refer you to what I have already said. There is in the treatment of acidity an advantage in giving mineral waters which does not exist in the treatment of diabetes. In acidity pure water may be regarded as an anti-acid. It lessens the symptoms, and prevents the consequences of the acidity; where strong acid irritates, weak acid has no effect. Strong acid quickly sets free uric acid; whilst weak acid has to make up by time what it wants in force, so that simple dilution, or the amount of water that exists in the mineral water, becomes an important means of relieving acidity.

When the acidity is excessive, all these different methods of lessening acidity must be used at the same time. To give acid in the food and to neutralise it by medicine is child's play, unless the amount of alkali taken exceeds the amount of acid eaten. The greatest possible effect can be obtained by stopping acid from going in, by removing acid by perspiration, and by the

stomach, skin and kidneys, and by neutralising the acid by lithia, ammonia, magnesia, or soda.—*Med. Times and Gazette, Feb.* 18, *and* 25, 1865, *pp.* 165, 191.

28.—ON THE TREATMENT OF THE OXALIC ACID DIATHESIS.

By Dr. H. Bence Jones, A.M., F.R.S., &c.

[The cure of the oxalic acid diathesis is to be obtained by promoting oxidation in the body. Were it not for the oxalic acid combining with lime the oxalic acid diathesis would be of very little consequence, being as it is such a slight aberration from health. Hence it is of importance to prevent as much as possible the entrance of lime into the system.]

It is quite impossible to stop all lime from going in or coming out of the body; and, therefore, by the anti-lime treatment, only a partial good can be done, and other methods of stopping the crystallisation must be adopted, or still better the oxalic acid diathesis must be removed.

The second method of treating the oxalate of lime deposit in the urine consists in stopping the formation of the crystals. Removing the oxalic acid diathesis would stop the crystallisation as surely as it would be effected if no lime could be allowed to pass out of the body; and though both these methods may be of use, a still more immediate effect on the crystallisation may be obtained by the mineral acid treatment.

Dr. Prout first ordered a mixture of one part nitric and two parts hydrochloric acid. This is called nitro-muriatic acid. It varies in composition according to the temperature at which the acids are mixed and kept, and according to the time they remained mixed before they are used. Sometimes it is a mixture of nitric and hydrochloric acid, or it may contain chlorohyponitric, NO_2Cl_2, and chloronitrous acid, NO_2Cl, or even hyponitrous acid, HO_4, and chlorine. It is, no doubt, possible that a mixture of these substances may be more beneficial as a medicine than any one of them used separately; and if this can be proved to be true, such a mixture should be used; but the progress of our knowledge of remedies depends on the use of each substance separately, in order that the greatest possible clearness may be obtained as to the effect of each remedy; and, as it is by no means proved that so-called nitro-hydrochloric acid is superior in any respects to the mineral acids taken singly, I consider that an advance in clearness of intention, if not in power of treatment, may be obtained by using each acid separately, according to the properties which each must be considered or may be found to possess.

And first regarding hydrochloric acid. As this is more especially the acid of the gastric juice, it seems reasonable that it should be prescribed, more particularly when there is feeble digestive power; after long illnesses, when animal food begins again to agree, it should be taken almost immediately before each meal of animal food. Thus the greatest immediate use can be got out of hydrochloric acid.

Secondly, regarding nitric acid. This acid was at one time strongly recommended as a substitute for mercury in syphilis and in the liver diseases of India. Generally, it may be considered to promote secretion and perhaps oxidation; and with this intention it should be given so as to be absorbed before food enters into the stomach. If taken one hour or two hours before meals on an empty stomach, the nitric acid acts on the system and not on the food.

Thirdly, sulphuric acid has been always used as an astringent to stop perspiration, to stop hemorrhage, to stop diarrhœa. It may be, therefore, regarded as the opposite of nitric acid in its action on the system; and to obtain the greatest action it also should be given in time to admit of its absorption before food is eaten.

Thus, then, rightly used, hydrochloric acid promotes digestion, nitric acid secretion, sulphuric acid constriction.

In addition to these special actions, there is a direct local action of these substances on the stomach. When used in small quantities, with care, they all probably render the stomach less irritable than it otherwise would be; but if the dose of any of them be too strong, it probably increases the sensitiveness of the mucous membrane, and may cause violent pain and spasm, increased chemical action producing altered mechanical and chemical actions in the mucous membrane of the stomach.

All these acids also have a distinct action on the urine. Although there is as yet no positive proof that dilute mineral acids increase the acidity of the urine at all to the same degree that dilute vegetable acids will do.

If the amount of sulphates, for example, in the whole quantity of water passed in twenty-four hours for three successive days when no sulphuric acid is taken is compared with the amount of sulphates in the whole quantity of water passed in twenty-four hours for the three succeeding days when sulphuric acid is taken, it is certain that dilute sulphuric acid taken in very large quantity does pass off combined or uncombined in the urine. The third day after half an ounce of dilute sulphuric acid, specific gravity 1·1084, had been taken daily, 43 ounces of urine were passed of specific gravity 1025·4. 12·95 grains of sulphate of baryta were precipitated from 1000 grains of this urine; when no acid was taken, 42 ounces of urine were passed of

specific gravity 1023·4. These gave 9·19 grains of sulphate of baryta per 1000 grains of urine.

All the mineral acids lessen the tendency of the oxalate of lime to form crystals in the urine. If two equal portions of urine are taken, and the minutest quantity of mineral acid is added to one portion, whilst the other is kept for comparison without acid, oxalate of lime may be found in the urine to which no acid has been added, whilst in the other no crystals will form. The same action may occur when these acids are taken as medicine, and with nitric acid there is the additional recommendation that the oxalic acid diathesis itself may, by the promotion of oxidation, be lessened, or, with perseverance, removed.—*Med. Times and Gazette, March* 11, 1865, *p.* 247.

29.—ON ACID GRAVEL.

By Dr. H. Bence Jones, A.M., F.R.S. &c.

Nature of the Gravel that is caused by Over-acidity.—Generally speaking, it may be said that all the albuminous substances of vegetable or animal food, before they are thrown out of the body, pass through the stage of uric acid. A particle of albumen absorbed into the blood from the food may become part of an organ and serve the purposes of life, and then undergoing change become resolved into uric acid, then into urea and oxalic acid, and finally into urea, carbonic acid, and water; or the particle of albumen may go through a shorter circuit, and without becoming part of any organ or texture it may be transformed into uric acid, and ultimately pass off as urea, carbonic acid, and water.

Thus uric acid, like sugar, may come from two sources—first from the food, and secondly from the textures; and as sugar is one of the results of healthy chemical action, through which, generally speaking, every particle of starch must pass, so uric acid is one of the substances through which each particle of albumen in health passes before it is thrown out of the body. When more food is taken into the blood than is wanted for the muscles, nerves, and textures generally, the excess is thrown out in a more or less perfectly oxidised form by the kidneys, skin, and lungs; it may be as uric acid, or if further changed as urea, oxalic acid, and water; or if burnt, as far as possible, as urea, carbonic acid, and water; or different portions of the albumen may be thrown out in all these different states at the same time.

The excess of uric acid in the urine of those who have just taken an excess of albuminous food, whether animal or vegetable, is very easy of proof.

The quantity of uric acid in the urine I found to vary (see Philosophical Transactions, part ii., 1849, p. 235)—

	Grains.	Grains.	Sp. gr.
Before food (mixed diet)	from 0·048	per 1000 of urine	1026·3
	to 0·17	,,	1023·1
After food it varied	from 0·39	,,	1021·0
	to 0·92	,,	1031·1
On vegetable food—			
Before food . . .	0·049	,,	1024·0
After food	0·636	,,	1026·2
On animal food—			
Before food . . .	0·049	,,	1024·8
After food	0·77	,,	1029·9
After three days of animal food only the highest amount was	1·022	,,	1027·8
After three days of vegetable food only the highest amount was	1·010	,,	1025·6

The second source of uric acid is in the muscles, nerves, and other textures of the body. Wherever albuminous subtances exist—that is in each portion of the body—integration is continually balanced by disintegration, and one of the products of this disintegration of albumen is uric acid, which, returns into the blood and, more or less oxidised, passes off by the kidneys, and is found in the urine when no albuminous food at all has been taken for many days.

Derived from either source, uric acid exists in combination with alkalies dissolved in a more or less alkaline fluid in the blood. In the cells of the kidneys and elsewhere a process of oxidation occurs; other acids are produced, and the alkaline urates are made into more acid urates, which pass out of the kidneys to the amount of six to ten grains of uric acid daily dissolved in an acid fluid which is continually varying in acidity. Hence the composition of the urates in the urine is continually differing at different times.

In a paper in the Journal of the Chemical Society for 1862, vol. xv., p. 201, I have shown that in health potass, soda, and ammonia are all combined with uric acid in varying quantities, and these mixed urates constitute the ordinary granular amorphous deposit which is soluble in the warm urine. In three analyses I find—

	First Analysis.	Second Analysis.	Third Analysis.
Uric acid .	94·36	91·06	92·11
Potassium .	3·15	3·78	5·06
Ammonium .	1·36	3·36	1·61
Sodium .	1·11	1·87	1·20

These acid urates are sometimes combined with uric acid, forming still more acid urates, corresponding to quadoxalates, from which a portion of the uric acid is set free even by washing the amorphous sediment with water. If any acid acts on the acid urates they are decomposed, and uric acid is set free more or less quickly, according to the strength and temperature at which the acid acts. Thus, if in the cells of the kidney by oxidation mineral, vegetable, or fatty acids are produced, the urates are decomposed either in the tubes of the kidney, the pelvis of the kidney, the ureter, or in the bladder. Hence any acid whatever except the acid of extreme oxidation—carbonic acid—may give rise to uric acid gravel. Whatever the acid that may be taken in excess in the food, or whatever the acid that may be thrown on the kidney by inaction of the skin, or whatever the acid that may be produced in the stomach or in the kidney itself, if it decomposes the urates it may give rise to uric acid gravel. Acid phosphate of soda is not able to set free uric acid from the urates: free acid must be present; and hence free uric acid is the most delicate test of other free acids in the urine.

Means of Detecting the Acid Gravel.—In contradistinction to white or phosphatic gravel, and to black or oxalic gravel, this may be called red or uric gravel; but colour, though striking, is apt to mislead. Pure uric acid is as perfectly white as phosphate or oxalate of lime, and, though uric acid attracts to itself the colouring matter of the urine, and so becomes reddish-yellow, yet there may be next to no colour to attract, and so the gravel may remain white, and then chemical reactions or microscopical appearances can alone determine the nature of the deposit.

The chemical test for uric acid is very characteristic. The smallest portion of sand or gravel heated with nitric acid at a gentle heat is immediately oxidised. Among the products is murexan, which gives, with ammonia, the richest purple murexid, and by this reaction uric acid may be always known. Thus, however, you cannot determine whether the uric acid is free or in combination. Chemically this may be partially determined by fully oxidising the sand or gravel; if, on combustion, an ash remains, the uric acid is combined with some fixed base; but ammonia is volatile, and thus the surest method of determining whether the uric acid is combined or free is to observe the deposit as it passes in the urine with the microscope. Moreover, thus a single crystal of uric acid can be recognised; a quantity much too small to be tested chemically can be determined microscopically with certainty. Of course, errors of observation may arise, but the practised eye will learn to recognise the various forms of uric acid crystals, and possibly the

peculiarity of form may lead to a knowledge either of the acid which set the uric acid free or of the composition of the fluid in which the uric acid was set free.

Having determined that free uric acid is present, it is of importance to know how soon the crystals form after the urine is secreted by the kidney—whether they form in or out of the body. For this the urine should be passed into a bottle or glass and examined immediately, and if no crystals are then seen, every few hours a fresh examination should be made; and thus the amount of acid which is acting on the urates may be estimated and counteracted.

Symptoms Produced by Gravel.—When the red sand only forms after the urine is passed, it causes no symptoms whatever; but the acid or acids in the urine, which after a time liberate the uric acid, these may produce irritability of the bladder or of the urethra. Over acid urine sometimes causes excessive frequency and urgency of making water. The mucous membrane may be made so irritable that inflammation and all the symptoms and appearances of violent gonorrhœa may be produced; or the muscular texture of the urethra may be irritated, and spasm ending in spasmodic stricture may occur.

These chemical irritations do not, strictly speaking, proceed from the red sand; they come from the cause of the red sand; but they are necessary accompaniments of the mechanical irritations which the gravel occasions; and these we have now to consider.

As the gravel may form in the bladder, the ureter, or the kidney, so the mechanical irritations may begin in these different positions, and the symptoms will vary according to the part that is irritated.

We will take the mechanical symptoms produced in the kidney first, although the formation of gravel in the kidney is a sign of the greatest degree of acidity, because the urine only remains a few seconds in the kidney, whilst it may remain some hours in the bladder. The acid in the kidney must be much stronger, in order to set free the uric acid, than it need be in the bladder to produce the same effect, because less power in longer time produces the same result as a greater degree of acidity in a shorter time.

As long as the gravel is small, and lies unmoved in the pelvis of the kidney, no symptoms occur. During rest no microscopical or chemical signs or general symptoms show the presence of a foreign body in the kidney; but if sufficiently rough motion is made, then the gravel moves in the kidney, and slightly scratching the surface blood is mixed with the urine. The injury to the pelvis of the kidney may be far short of causing pain or visible blood; there may be so little that chemical test for

albumen may give no reaction of that substance; but yet the microscope may show distinctly a very few blood globules. These may come with motion and cease with rest. With much motion, much blood will come, and be visible in quantity in the urine; and this may occur without the least pain. So that I often ask my patient with hæmaturia from this cause whether he would know that anything was wrong if he were blind, and the answer is usually positively no.

Gradually as the gravel increases in size in the kidney, slighter motion brings blood, and pain, and uneasiness in the loin. Distension of the colon, with flatulence, when it presses on the kidney, causes considerable pain.

If the gravel does not pass into the ureter, very little pain arises until the increase of size leads to pressure on the kidney and absorption of structure. I have seen cases in which I have no doubt that calculi have been in the kidney for very many years. In proof of this I could mention, from among many others, cases of seventeen, twenty, and probably of fifty years' duration.

The symptoms produced by gravel in the ureter are much more severe than those produced by gravel in the kidney. The instant the gravel or stone produces mechanical pressure violent pain comes on. The funnel form of the commencement of the ureter, allows the gravel easily to escape a short distance from the kidney; and hence after some sudden jerk or even a turn in bed the pain begins suddenly and is referred to the kidney itself. Through nervous sympathy, vomiting soon follows, according to the size of the gravel or stone and the size of the ureter. The narrow passage is more or less quickly passed, and with a slight obstruction at the still less elastic entrance into the bladder the mechanical impediment is over, and the symptoms cease.

The symptoms produced by gravel or small stone in the bladder correspond with those produced in the kidney. At first there are no symptoms at all; violent exercise may produce microscopic appearance of blood, but until the gravel tries to escape, or is thrown by contraction of the muscular texture on the neck of the bladder no pain is felt. Gradually as the stone increases, or the bladder becomes irritated and wants to contract frequently, blood becomes visible, and pain on motion and on emptying the water becomes severe. This pain is referred to the extremity of the nerves at the end of the penis. The flow of urine is momentarily broken by the gravel or stone stopping the passage. If the stone is small enough or the urethra large enough the foreign body escapes into the tube, and in a longer or shorter time it is driven out.

Although gravel may stop in the urethra and gradually

increase there until mechanically removed, yet it is very unlikely that it should begin to form there. If the crystallisation commenced there the passing urine would rapidly wash the crystals away before they caused any symptoms; so that the urethra does not suffer primarily from wrong chemistry. But the peripheral pain of the glans penis, and the scalding of the urethra from the acid urine, and the mechanical injury which the sharp fragments of acid stones occasionally produce in the urethra after lithotrity; these all show that even the penis does suffer from mechanical secondary disorders arising from primary chemical complaints.

Treatment of Acid Gravel.—In the treatment of every kind of gravel the first question is regarding its solvency. The solution of uric acid gravel in any alkaline fluid that can be borne in contact with the mucous membrane of the urinary organs is scarcely possible. It can hardly be doubted that uric acid in a fine state of division as powder could be dissolved, but the problem is to dissolve uric acid in a more or less massive state. The mechanical force of cohesion of the crystalline particles as well as the chemical insolubility of each crystal has to be overcome. And notwithstanding the appearance on the surface of some uric acid calculi, and notwithstanding the supposed disappearance of calculi that have been considered to be in the urinary organs, and notwithstanding I have made numberless experiments, in the body and out of the body, to find a solvent for the stone in the kidneys or in the bladder, yet at present the state of aggregation of masses of uric acid cannot be overcome chemically. Acid gravel has not been dissolved in the kidneys or bladder, though my friend Dr. Roberts, of Manchester, tells me he expects to do both.

Until the gravel comes away or is removed, two chief indications for practice exist. Firstly, to prevent it from hurting the mucous membrane, and secondly to stop its enlargement.

Pain and blood are the evidences of hurt to the mucous membrane. Hence, any amount of exercise or of motion may be taken, provided it does not cause pain or blood. The amount of pain may be taken as the measure of the risk of inflammation, and the amount of blood may also be taken as the measure of the mechanical hurt to the kidney, and of the loss of strength which results from hemorrhage. Both pain and blood may be prevented or temporarily removed by rest; but long-continued perfect rest would in most people soon injure the general heath. So that the mean must be found by trials, the problem being to keep up the general health by exercise, but to avoid by rest inflammation or hemorrhage—that is, chemical or mechanical injury.

The second indication is to stop the growth of the stone.

In other words, to prevent fresh acidity; for thus the gravel is kept so small as to admit of perfect cure by the escape of the mechanical mass through the urinary organs. The means of preventing acidity have been already brought before you. Here I shall only say a few words on the good of dilution, which not only delays the precipitation of the uric acid, but helps to wash out the gravel or stone. The best diluent is some alkaline mineral water, as Vichy, or Fachingen, or Seltzer water, taken not only with the food, but two hours before each meal. When there is no acidity, soft spring water night and morning is the best diluent.

This brings me now to the treatment of the passage of gravel or stone from the kidneys or bladder. The chief objects to be obtained are, first, to relieve pain; and secondly, to promote the passage of the calculus.

The pain is most rapidly removed by chloroform or ether inhalation. This, with care, may be continued for hours. The slightest unconsciousness is all that is necessary; but this must be kept up, for on the return of consciousness the pain is again felt.

The second remedy is the hot-bath at the temperature of 100° F. continued for hours, unless it causes faintness. Four hours' bath and then two hours' warm blankets; then three and a-half hours bath and three hours' warm blankets; continue in this manner, gradually prolonging the rest and shortening the bath. Nothing weakens like pain. Or any hot applications may be used when the bath cannot be taken.

The third remedy for pain is opium externally or internally.

Externally, it may be used as a fomentation in any quantity; as laudanum or solution of morphia poured on a poultice, or with one-eighth to one-sixteenth of chloroform on spongio piline. Internally, half a grain of acetate of morphia in solution may be injected into the loin; within a few minutes it will act potently on the pain; or opium, or any of its preparations, may be given by mouth freely, and the severity of the pain renders the system tolerant of large doses; but the ill effects of the sedative in causing inaction of the liver and constipation are very evident when the pain ceases. Half a grain or even two-thirds of a grain of morphia may be given by the mouth, and repeated in four or six hours, if the pain lasts; or thirty drops of laudanum may be taken, and repeated in two or three hours. For the relief of the suffering it is best that no aperient should be taken at the same time as the opiates, the action of the aperient counterbalancing the action of the narcotic on the bowels. Calomel in full aperient doses, as it stops sickness, may be given with the opium if there is great constipation, or if there is great intolerance of narcotics.

Secondly, the passage of the calculus is probably made easier by all the means that relieve pain. When the spasm is relaxed by chloroform and opium, or by the warm bath, or fomentations, the escape of the calculus is made more easy. Still, the pressure of the urine behind the obstruction is the great motive power in the passage of the gravel or stone. This force comes into action only when the obstruction is more or less complete, and then the pressure of the fluid is the same on the structure of the kidney as on the obstruction; and thus the secretion of urine is lessened and the pressure more and more slowly reaches its maximum. Diluents may early in the attack be of use in increasing the pressure; but after the symptoms of suppression of urine occur, they are of less use in this respect, and when urinous poisoning comes on vomiting and purging give relief by removing some of the poison. Usually, without emetics or aperients, sufficient, sometimes excessive, evacuation takes place; and if this tertiary chemical poisoning does not bring the heart to rest, the obstruction of the ureter may be removed by the continued pressure, and then, the circulation through the kidney being free, the blood is rapidly purified, and a chemical disease far more dangerous than the original chemical complaint (acidity) rapidly passes away.

When the obstruction cannot be removed, not only does the pressure of the secreted urine stop the purification of the blood by stopping secretion, but the urine that has been poured out must, by diffusion, pass back into the blood. When the blood is urinous, diffusion must carry the poison to every part of the textures, even into those parts where no vessels exist—the cornea, the crystalline lens, the cartilages, and perhaps, even into the nails and the hair. The uric acid, oxalic acid, and urea poison the nerves, and muscles, and blood-vessels, and other textures, and render them incapable of carrying on the actions of nutrition and oxidation.

When the gravel passes into the bladder, usually little or no treatment is requisite. After a longer or shorter delay the stone is forced into the urethra, and ultimately is driven out; but whilst it remains the same objects are still to be kept in view—first, to promote the escape of the stone; secondly, to prevent its growth; and, thirdly, to keep the bladder from harm.

Free dilution and alkaline remedies best fulfil the first two objects; whilst the last is chiefly to be gained by rest. Inasmuch as the bladder is nearer the external orifice than the kidney, two or three other remedies are possible.

First, there is great surgical authority for occasionally passing a catheter when a small stone is suspected to be in the bladder. If there be no stricture, it is not easy to see how this

should help the passage of the stone more than passing a probang before food would make swallowing easier.

A gentleman came to me one morning, and from the microscopic blood in the urine and the pain in making it, whilst his general health was robust, I considered a stone had formed in the bladder. I went with him to a surgeon, who sounded him, and said "There may be a stone, but I cannot find it." The following morning the prtient brought me the stone, which he had voided in the night. To him, and to his surgeon, it was beyond all doubt, that the passing the sound made the stone come away.

Secondly. The Astley Cooper forceps, by which a small stone may be removed whole through the urethra; and the lithotrite, by which a stone may be removed in pieces; and, still more, lithotomy, by which the foreign body is removed whole through an artificial opening, are mechanical proceedings belonging to the surgeon and not to the physician.

Thirdly. There is yet another method of cure, so far beyond all other methods in its philosophy, its safety, and its freedom from suffering, that success, though still postponed, must ultimately be attained. This is the dissolving the gravel in the bladder. There is no difficulty whatever, excepting want of patience and chemical knowledge, that hinders the solution of phosphates in dilute acid. A stream of diluted lactic or hydrochloric acid, made so weak as not to irritate the bladder, and passed at the temperature of 100° F., must dissolve any phosphatic gravel or stone or outer layer of stone, provided only that the current is continued long enough.

A warm stream of very diluted lithia or soda should dissolve any urates and attack uric acid. But gravel and stone vary very much in density, that is, in compactness of aggregation, and some uric acid gravel is so hard that it is vain to expect that any stream of water or of alkaline water will have any effect in any reasonable time.

Hence it becomes necessary for the solution of some uric acid stones, and for all oxalate of lime stones, to discover some way of exposing the gravel or calculi to the action of stronger agents than dilute lithia or soda.

In the Philosophical Transactions for 1852, p. 201, I have shown what can be done out of the body by disengaging nitric acid and potassa in contact with uric acid by the help of the voltaic battery. Although this is easy in glass vessels, it is very difficult to make a catheter that can cause this action to take place within the bladder, and after months spent in producing an instrument fulfilling all the electrical conditions for success, the equally essential surgical conditions are by no means perfect. There is a want of firmness and fixedness of

the blades which make the introduction of the litholitic instrument difficult. Still, when surgeons skilled in electrical science, and possessed of sufficient time and patience, arrive, I have little doubt but that uric acid calculi will be dissolved in the bladder. Some more energetic substances may be liberated in contact with the stone, or some way may be found of rendering the stone more soluble. At present progress is stopped more by the mechanical than by the chemical difficulties of the problem.

Oxalate of lime gravel or calculi are so hard and so insoluble that as yet there is no prospect of obtaining any good results by any process of solution. But oxalate is so closely related to carbonate of lime, and this last is so rapidly soluble in any dilute acid, that hopes must be held firm that some day a process of oxidation may be carried on in the bladder to compensate for the want of that oxidising action in the blood and organs which leads to the formation of this kind of stone.—*Medical Times and Gazette, March* 25, *and April* 1, 1865, *pp.* 301 *and* 328.

30.—ON PHOSPHATIC GRAVEL AND CALCULI.

By Dr. George Harley, Professor in University College, London.

[Deposits of phosphates may take place in the kidney, giving rise to renal calculi, but they more commonly take place in the bladder. The pathology of the two is very different. Thus, renal phosphatic calculi arise in one of two ways,—either from the urine containing an excess of phosphates, in consequence of a vice in the tissues which contain them; or from the urine being alkaline at the moment of its secretion in consequence of being secreted from abnormally alkaline blood. In the case of vesical calculi, in addition to the causes just mentioned there exists a fruitful source of origin in the numerous diseased conditions of the bladder itself.]

As regards the first step in the formation of calculi, it may be laid down as a general law that *their formation in any particular part of the urinary system is due to some special local cause.* If the calculus begins in a urine tube, it is on account of the tube getting blocked up with mucus, epithelium, or a little blood. Or it may also arise from the tube being temporarily constricted in some parts of its course in consequence of inflammatory action. Once, however, that the deposition of urinary sediment has begun, the original cause is soon lost sight of, and the calculus goes on forming round its nucleus quite independent of the local condition which first called it into existence.

When a phosphatic calculus, instead of beginning in a urine

tube, originates in the pelvis of one of the kidneys, there may be no absolute obstruction to the flow of the urine, but merely a slight interruption, as from a fragment of mucus adhering to the side of the lining membrane of the pelvis. For when a predisposition to calculus formation exists it requires but a very trifling cause to call it into operation. Almost anything may form a nest round which the stone will gradually become deposited.

This is no fanciful description of the formation of renal calculi, as you may see by taking the trouble to examine the series of examples now on the table, each of which bears evidence of the truth of the statement. In fact, the assertion might be proved without any specimens at all, from the simple circumstance of the frequency with which we meet with renal calculi limited to one kidney in patients predisposed to stone. That fact of itself clearly shows that the deposition of the stone in any one particular spot must be entirely due to some local cause. Thus, for example, we have here a kidney, which contains no less than fifty-nine uric acid calculi, and yet neither in the bladder nor the opposite kidney of this gentleman was a single stone to be found. The deposition of all these calculi in one kidney must therefore have been entirely due to some local cause. At first sight, the cause does not appear very evident, nor is it apparently easy to account for the fact that although at least fifty of these calculi are not bigger than barleycorns, none of them have found their way into the ureter, and from thence onwards into the bladder. The reason of the last fact is, however, apparent when the specimen is carefully examined, and as it will be easier to explain the former proposition after we have discussed the latter fact, we shall consider the last first. The largest stone, which, from its position and size, is evidently the one which was first deposited, is of a very peculiar conical shape, and the apex of the cone projects into the orifice of the ureter, and forms a movable plug, which is, in fact, a self-acting valve. No sooner does the pelvis of the kidney become filled with fluid, than its distended walls cease to embrace the stone, and consequently the fluid flows past the valve until the pelvic walls again come into contact with it. The separation of the walls from the valve, although being sufficient to allow liquid to pass, does not appear to be sufficient to admit of the escape of even the smallest of the stones. Now exactly the same thing as we have done artificially with water must have been daily done with the urine.

Gradually as the urine was secreted, it would distend the pelvis of the kidney and separate its walls from the valve-shaped calculus; but no sooner would this occur than the accumulated urine would make its escape. So that every now and

again there would be little accumulations of urine, and during the period of these little accumulations not only would the smaller calculi form, but the larger one, the plug, would gradually increase in size. In this case, it is perfectly evident from the immense number of the calculi that the predisposition to stone existed; and from their being all limited to one kidney it is just as evident that it was some local cause which induced them to select the left instead of the right kidney. In Dr. Roome's case, again, it was precisely the reverse: all the calculi were found in the right kidney; not one existed in the left; which is additional evidence in favour of our views.

In my own case, moreover, it is further shown, by the absence of all calculi both from the right kidney and from the bladder, that, although the predisposition to stone existed, yet without some local cause no stone would have been deposited in the urinary passages of the patient—a most suggestive fact, as showing the importance of paying attention to the proper performance of the renal function. Who can tell what a trifle at first induced the deposition of the uric acid nucleus, which ultimately became the plug and caused the formation of all the rest?

The foregoing general principles are not alone applicable to the pathology of phosphatic, but to all varieties of renal calculi —oxalic, uric, xanthic, &c.

As regards the first step in the formation of vesical calculi, it may, with equal justice, be said that whenever the predisposition to stone exists, it requires but a very trival cause to bring it into operation. For, as with renal calculi, almost anything may act the part of a nucleus round which the stone will become deposited. An insignificant particle of mucus, a microscopic portion of albumen, or an epithelial cell, is all that is requisite for the purpose. In the vast majority of vesical calculi, indeed, it is impossible to detect the primary point round which the urinary salts become adherent; but every now and then we come upon calculi with well marked nuclei, some of which are not only quite foreign to the urinary passages, but even foreign to the body itself. Thus, for, example, this large ammoniaco-magnesian stone (No. 3425 of Liston's collection in the museum of University College), which was extracted from the bladder of a man aged sixty, has a piece of beeswax for its centre; while three other small calculi (No. 3478), removed from the bladder of a man aged seventy, have barleycorns as their nuclei. It is perfectly clear these substances were not generated spontaneously in the urinary passages, and it is highly probable also that they did not find their way there quite accidentally; but with the mode of entrance we have at present nothing to do; so we may turn our attention to this other

vesical calculus (No. 3515 of the same collection), which was removed from a woman's bladder. The nucleus of this stone consists of a piece of bone—a fœtal tibia, in fact—which found its way by ulceration into the bladder from the abdomen, the case having been one of extra-uterine fœtation. Any solid substance, indeed, may prove the starting-point of a vesical stone, just as a thread, piece of string, or anything else, may form the nucleus of a crystal of sugar-candy.

In connection with the origin of calculi, there is another point particularly deserving of attention—namely, that local causes are the most potent in the production of stone, and that even where the predisposition to it exists, without some local cause no stone is likely to form.

Vesical phosphatic calculi are frequently induced by inflammation of the bladder. The inflammation gives rise to a copious secretion of mucus, which, acting as a ferment, causes the urea to decompose, while the urine still sojourns in the bladder. The decomposition of the urea leads to the evolution of ammonia, which, in his turn, deposits the earthy phosphates, and the gradual formation of a stone is the result.

Many phosphatic calculi have a uric acid nucleus, while some few have oxalate of lime for a centre. The deposition of phosphates, and consequent increase of the stone in such cases, is due to one of the four following causes:—

1st. Although the patient has recovered, either by time or by treatment, from the uric or oxalic acid diathesis, the stone that has already formed still remains in the bladder, and immediately, or at some future date, causes such an amount of irritation as to induce an excessive secretion of mucus; which mucus, acting as a ferment, brings about the decomposition of the urea, and consequent formation of the alkaline carbonate of ammonia, which, in its turn, induces, as in the preceding examples, a deposition of the earthy phosphates normally present in the urine.

In this case the deposition of the phosphatic calculus is not due to a vice in the general system, but the product of an entirely local cause.

2nd. A patient with a small uric or oxalic acid stone in the bladder, which induces no irritation whatever, is on some occasion or other seized with retention of urine. It may be from stricture, enlarged prostate, paralysis of the bladder, or any other cause liable to induce it. In consequence of this retention the urine becomes decomposed, and a deposition of phosphates takes place round the nucleus, the presence of which until now was of no importance whatever. The deposition of phosphates is, however, copious, and the stone rapidly increases in size, and becomes of itself a source of irritation. Here, then, neither

the original nucleus nor the constitution of the patient is to blame, but a local cause quite independent of either.

3rd. A deposition of phosphates may take place round a nucleus of uric acid, oxalic acid, or anything else, not on account of its setting up irritation, not because of the urine being retained and decomposed, but from there being an excessive elimination of phosphates by the kidneys. In common language, on account of the patient labouring under the phosphatic diathesis.

Lastly. A phosphatic calculus may form round a uric acid or other nucleus without any of the already-mentioned conditions existing, but simply and solely from there being an absence of that normal acidity of the urine which is essential to the retention of the earthy phosphates in solution until its emission from the bladder.

The diagnosis of cases of phosphatic calculi, renal as well as vesical, is not only greatly aided by an examination of the urine, which may be said to contain, as a general rule, less phosphoric acid than normal but even the differential diagnosis of the various causes inducing the calculi is also aided by an examination of that liquid.

1st. In cases arising from an excessive elimination of phosphates the reaction of the urine is normal, and it has no ammoniacal odour, nor does it deposit an excess of mucus.

2ndly. In those arising from an abnormal alkalinity of the blood the urine, although alkaline at the moment of its emission, either immediately after meals or during the whole day has no ammoniacal odour, nor does it deposit mucus in excess.

3rdly. In the first and second class of cases the urine is ammoniacal as well as alkaline when passed, and usually yields a copious deposit of mucus.

Lastly. The first and second class of cases are readily distinguished from each other by their histories.

Besides the triple or ammoniaco-magnesian phosphate, which is the most common kind of phosphatic calculus, there is also another variety composed solely of the simple phosphate of lime. This is comparatively speaking a rare form of stone, and fortunately its appearance is so very peculiar that it can be readily recognised without an analysis. Its external surface is smooth, polished, and shining like glass. It is exceedingly brittle, and has the peculiar tendency to break into regular layers like an onion.

Treatment.—From what has been already said it must have been seen that the various classes and kinds of phosphatic calculi each demand a separate mode of treatment, and that the old method of treating all kinds of phosphatic calculi alike is radically wrong. The pathology is the guide to the treatment.

In the first class the stone itself has to be attacked; in the second class the cause of the retention is to be removed; in the third class the vice in the tissues has to be got rid of; while in the fourth the normal acidity of the urine is to be restored. One and all of these are more or less within our power.

First, as regards the class of cases where the deposition of phosphates depends upon a decomposition of the urine taking place in the bladder. That we have seen may originate in two ways, and, as we shall again have occasion in another lecture to refer to the mode of treating both of the causes above specified, we shall for the present take it for granted that the cause has been removed, and that all that has now to be done is to get quit of the gravel or stone that has already formed in the bladder.

The common way to remove a vesical stone is, of course, to cut or crush it; but many persons, and especially old people, in whom the phosphatic calculus is most common, object to either of these operations being performed. What, then, is to be done? Fortunately, in the case of phosphatic calculi, the physician has much in his power, for of all calculi the phosphatic are the most soluble, and if only gravel has as yet formed the chance of geting it removed by dissolution is very great. The simplest mode of treating such cases is to keep the urine constantly acid by the administration of the nitro-muriatic acid three times a day, after food, and to irrigate the bladder by means of the double irrigating catheter. This is a much easier operation than most persons who have not tried it imagine. The patient being placed on his back, the double catheter is passed into the bladder, the urine allowed to flow out, and then a caoutchouc tube, at least four or five feet long, is slid over the end of the catheter, while the free end of the tube is dipped into a can containing water acidulated with nitro-muriatic acid. The can having been previously placed at a convenient height above the patient—on the top of the bed-post, for example—the caoutchouc tube is to be so arranged that when the patient is ready the acidulated water will flow in a regular stream down through the tube, along the catheter, into the bladder, and from the bladder back again through the other division of the catheter, along another caoutchouc tube attached to its opening, into a pail placed beneath to receive it. If the acidulated water be kept at a temperature of about 90° F., the stream through the bladder may be kept up for a long time without causing the slightest discomfort to the patient. When the top pail gets empty it is refilled from the other as often as thought necessary. This treatment being continued for a certain time every day, the gravel is gradually dissolved without causing the patient any suffering.

In the third set of cases strict attention should be paid to the general health, and an effort made to improve it, and thereby restrain the elimination of phosphates from the system by a judicious selection of tonics. If this is not effectual and the daily waste excessive, superphosphate of lime is to be administered, for the reasons previously given.

In the fourth class of cases, where the vice lies in the abnormal alkalinity of the blood, the strong mineral acids are to be administered. Vegetable acids, on the other hand, ought to be avoided, from the fact of their being transformed into carbonic acid in the system, which only tends to increase the mischief. In general it is sufficient to give the acid after food, but in very bad cases it is sometimes necessary to administer acid tonics also between the meals. In many of these cases the patients perspire freely, and the sweat is excessively acid, which in some measure accounts for the diminution of acid in the blood. To this class of patients I find much benefit arises from the use of nitro-muriatic acid in conjunction with gentian after meals, and a dose of sulphuric acid, to diminish the perspiration, between them. The patient's diet must also be carefully regulated; the animal food increased, the vegetable food diminished. The reason of this is plain enough, if what was said in the first lecture regarding the influence of diet on the reaction of the urine be considered. Herbivorous animals have alkaline, carnivorous animals acid urine, so that the more the patient resembles the carnivorous animal in his diet the less alkaline his blood, and the more acid his urine will become.

In trying thus to check the formation of the gravel o stone, care ought at the same time to be taken not to run to the opposite extreme, and by increasing the acidity of the system too much produce a deposition of gravel or stone of another kind. Uric acid, for example. The safeguard against falling into this error is found in the condition of the urine. As soon as the normal acidity of the urine is permanently restored the acid treatment and animal diet is to stopped.—*Medical Times and Gazette, Nov.* 5, 1864, *p.* 483.

31.—ON BRIGHT'S DISEASE.

By Dr. William Jenner, F.R.S., Physician in ordinary to H.M. the Queen and H.R.H. the Prince of Wales.

[Dr. Jenner commences with a consideration of the general facts in morbid anatomy which have an important bearing on the intrepretation of the lesions of structure of the kidney in Bright's disease.]

During life, such portions of animal structures as are not

absorbed when they cease to be duly nourished, either calcify (i.e., petrify), or they undergo, first granular disintegration, and then fatty metamorphosis, or degeneration as it is called. Chondrin and gelatin yielding structures more commonly calcify; protein compounds more commonly experience disintegration and fatty degeneration. In the last case there is a re-arrangement of the ultimate elements of the part, carbonate of ammonia and olein being the results. The protein compounds which experience these changes may be physiological or pathological structures or products. The cells of the kidney, the blood, the vessels, are all physiological structures, made up more or less of protein compounds; and it is matter of observation that, when any of these parts are so damaged as to cease to perform function, they undergo granular disintegration and fatty metamorphosis.

Lymph, a pathological product, when exuded into the kidney and not developed into tissue, undergoes the same disintegration and metamorphosis.

Varieties of lymph.—Under the word lymph are included various protein exudation matters.

One variety of lymph, quickly after it is exuded, disintegrates, forming innumerable molecular granules, which themselves rapidly undergo fatty degeneration.

Another variety of lymph has for its great characteristics rapid diminution of bulk, or contraction, forming itself, as it contracts, into imperfect fibres.

A third variety retains its transparency, homogeneousness, and bulk for a very long period.

And a fourth variety developes or is developed into those fibre-cells which Lebert so well described, and to which he gave the name of fibro-plastic corpuscles and fibres.

The exciting cause of the pathological lesion on which the exudation depends, the part into which the exudation occurs, the rapidity with which it takes place, and the constitutional state of the patient at the time it exudes, all appear to have an influence in determining the special variety of lymph that shall be exuded in any given case. And as several of these conditions vary during the same illness, you will understand why it is that although one or other of these forms of exudation may preponderate in any given case, the others in less quantity are found commingled with it.

For the sake of brevity, we may designate the

1st variety Granular lymph.
2nd ,, Contractile lymph.
3rd ,, Albuminoid lymph.
4th ,, Fibro-plastic lymph.

These varieties of lymph present another difference of great

pathological importance—viz., that while the first and the third have considerable penetrating or transuding power, and therefore infiltrate every texture in their vicinity, the second and fourth have, like the blastema of tubercle, very little penetrating power, and are found near only to the part at which they have been exuded—they do not infiltrate the textures in their vicinity.

To determine during life the size, tenderness, &c., of the kidneys.—In a perfectly healthy but moderately thin person the kidneys may be readily felt by the hand. The left, partly from its being longer, is more easily handled than the right. Neither tenderness nor moderate enlargement can (except in the very fat) escape detection. To feel the left kidney you place the fingers of the right hand directly under the last rib, *just* outside the *mass* of lumbar muscles, and the points of the fingers of the left hand *just* inside the corresponding spot in front; the kidney is between the two. Depress the fingers of the left hand, and at the same time raise the points of the fingers of the right hand, and you will have no difficulty in feeling the kidney, in defining its outline, its mobility, &c. This mode of palpation is far more practically useful than is percussion for determining the size of the kidneys.

Special pathological conditions of the kidney which may lead to Bright's disease.—I proceed to pass in review those lesions of structure which affect every part of both kidneys, and which, if not rapidly cured by nature or by art, are accompanied by albumen in the urine, by anasarca, and by retention of the urinary elements in the blood.

Congestion.—Although now and then some tumour, as the gravid uterus, presses on the renal veins, and so mechanically causes over-filling of the veins of the kidney, still by far the most common cause of this form of passive congestion is impediment to the circulation through the heart and lungs.

When the blood is unable to escape freely from the veins of any organ, the first vessels to yield to the pressure of the blood on their inner surface are those which have the thinnest coats and are the least supported by the textures in which they are imbedded. The capillaries of the Malpighian bodies are at once the thinnest coated and the least supported of any part of the vascular system of the kidney.

Incomplete impediment to the return of blood from the kidney has, then, as its first perceptible result, over-filling of the capillaries of the Malpighian bodies; if the impediment continue, then the stretched walls of the vessels permit serosity poor in albumen to escape. The impediment being continued, the next result is the escape of serosity containing more albumen and some fibrin in solution—i.e., of liquor sanguinis poor in

fibrin; and the impediment continuing, the final result is rupture of the over-stretched walls of the vessel.

If the impediment be considerable and rapidly formed, then rupture of the over-filled vessel is the almost immediate result; the rule being that the more gradually the impediment is formed the less likely is rupture to occur. The state of the blood has a decided influence in determining the character of the effusion; for if the patient be very anæmic (or to use the last-devised name for what we formerly called anæmic — oligoerythrocythæmic and hydræmic), then the escape of serosity is considerable, the serosity of watery blood, poor in red corpuscles, readily escaping through the walls of the capillaries. On the other hand, if the blood be rich in red corpuscles, then rupture of the capillaries is one of the earliest consequences of a very moderate amount of impediment.

Casts of tubes; blood-casts.—When blood is extravasated into the tubules of the kidney, some portion at least coagulates before its escape into the pelvis of the organ; coagulates, that is to say, in the tubules. The fluid poured out by the vessels of the Malpighian bodies washes out the coagula so formed, and the expelled coagula are to be detected with the aid of the microscope in the urine. As these little coagula are moulded to the inside of the tubules, they are of the shape and diameter of the tubules in which they are formed. They are therefore really casts of the tubules. Their diameter will vary according as the tubules of which they are casts are denuded of their epithelial lining or not, and according as they are formed in tubules of the cortex or of the pyramids.

Their number will vary with the quantity of the fibrin or blood effused, the number of the Malpighian bodies from which it has escaped, and the rapidity with which the blood coagulates, and with the pressure exerted on them by the fluid poured out from the vessels of the Malpighian bodies.

If the fibrin adhere, as it sometimes does, to the cells lining the tubules, then those cells may come away with the coagulum, and constitute a great portion of the cast. If a little serosity escape from the portal capillaries, and then pass, as it must, through the basement membrane of the tubules, it will loosen the attachment of the cells to that membrane and to each other, and so favour their expulsion. Again, after serosity has passed for some time through the tubules, the cells will be to some extent infiltrated by it, their structure will be damaged, and their function impaired.

The coagula retained in the tubules, the cells damaged by albumen passing into them, the cells the attachment of which has been loosened, and the basement membrane itself injured by the passage of the albuminous fluid, will all, in accordance

with general laws, undergo granular disintegration and fatty degeneration.

Before leaving the subject of passive congestion, let me point out and impress on you that chronic impediment to the flow of blood through the lungs and heart is always intermitting in degree—now almost none, then extreme, then little, and again extreme; and the congestion dependent on this impediment must also intermit or remit.

Acute passive congestion of the kidney is not unfrequently the result of feeble action of the heart, gravitation, and a state of blood unfavourable to its passage through capillary tubes. In typhus fever, in typhoid fever, and in diseases of typhoid type generally, congestion of the kidney from these causes is not of uncommon occurrence. The kidneys are not the only parts which suffer congestion under the conditions mentioned; the posterior part of the lungs, and the skin of the back generally, suffer first. In the skin we see all the changes in the capillary circulation; a faint bluish tint of the surface, with a dusky hue of that of the back; then an almost purple colour of the skin of the back; and finally the latter is studded with petechiæ, or little extravasations of blood. If we press the fingers on the skin of the patient, we remove the blood from the compressed part, and cannot fail to be struck, not only with the change of colour, but also with the time that elapses before the blood re-enters the capillaries.

In these cases of passive congestion of the kidney blood is usually present in the urine, and albumen out of proportion to the quantity of blood. As the blood in diseases of very low type—e.g., so-called malignant typhus fever and erysipelas—is little disposed to coagulate, casts of tubes may be absent, or present in small numbers only.

Passive congestion of the kidney, especially the form last described, is very often followed by reaction and the establishment of active congestion, and finally of the exudative stage of inflammation. The lymph exuded is the granular variety. Nutrition being interfered with, granular disintegration of the contents of the cells occurs. When reaction occurs, a febrile disturbance of low type is the result of the state of the kidney and of the resulting uræmia, and the primary affection is apparently prolonged beyond the date at which it should have terminated. The reaction which ensues on this form of passive congestion, and the low-typed inflammation that follows, have long been studied as they occur in the most depending part of the lungs.

Primary active congestion of the kidney—i.e., the first stage of inflammation—that stage which precedes exudation—is a common consequence of exposure to cold, and of the state of the blood

in scarlet fever, and sometimes, though more rarely, of the state of the blood in other acute specific diseases—pyogenic fever, pyæmia, septicæmia, &c.

In congestion the result of direct mechanical impediment to the return of blood from the kidney—e.g., from disease of heart or lungs—the Malpighian vessels, as we have seen, are the first to become over-distended, and by permitting the escape of serosity, of liquor sanguinis, or of blood, may retard, moderate, or altogether prevent the over-distension of the vessels of the portal system—i.e., of those veins and capillaries which lie between and around the tubules. In the passive congestion that occurs in typhoid diseases, and still more in active congestion, there is no such exemption of the renal portal system; nay, it seems probable that the intertubular vessels are the special seat of active congestion.

If active congestion subside before lymph has been exuded—i.e., if serosity only has been poured out, then, although a considerable loss of epithelium will have been necessitated (as the cells are renewable), no decided permanent damage to the structure of the kidney will have resulted. On the subsidence of the active congestion the patient will be said to be well. The loss of epithelium will have been necessitated because, the vessels between the tubules being affected, serosity will have been effused there, transuded the basement membrane, and loosened the attachment of the cells to it and to each other. The patient, I say, will be *called* well, because, although parts once damaged are never permanently restored, still they may be so far restored that we are unable to detect any inability in them under ordinary conditions to perform function. Still forget not that these little injuries do remain, and when some extra strain on the damaged organ comes, they tell on the result. There can be no doubt that these trifling "damages" to structure are one reason why certain diseases are so fatal to those of advanced life. Now this organ, and now that, is damaged to a trifling degree, until at last although the man is "in health," none of his organs are sound.

I have described or defined active congestion to be the stage of inflammation preceding exudation; but such a stage is of most limited duration, and it is probable that in the serosity exuded almost if not quite from the first there is some fibrin in solution. Still, from a practical point of view, it is important to separate the two stages. For when exudation in quantity has occurred, then the evidences of disease are manifest and the patient becomes the subject of chronic Bright's disease.—*Lancet, Jan.* 14, 1865, *p.* 29.

32.—ON THE PRINCIPLES OF TREATMENT OF BRIGHT'S DISEASE.

By Dr. Arthur Hill Hassall, Senior Physician to the Royal Free Hospital.

I have to make a few remarks relative to the principles upon which Bright's disease should be treated. What these principles are we shall experience but little difficulty in determining.

We must remember that in two cases related by me there has been a very large drain of albumen; that the blood has consequently become impoverished; that its serum is so deficient in albumen that, in obedience to the law of endosmosis, it readily transudes from the containing vessels, and makes its way into the surrounding tissues and cavities, giving rise to the anasarca which was present to so large an extent in these cases on their first admission; that the system, therefore, is permeated, and the various functions of the body impeded and deranged by the presence of this excess of water.

That coincident with this loss of albumen there is also diminution of the red blood-corpuscles, as shown by the analysis given, and probably also of the fibrin. Of this destruction of the red blood-corpuscles the peculiarly pallid aspect of the face in both patients alone affords a sufficient proof.

That in the case of G—— the kidneys are in a state of fatty degeneration; and that, owing to the structural changes which have taken place in those organs, there is diminished elimination; and that consequently the dangers of uræmic poisoning and convulsions are to be apprehended.

I reiterate, therefore, that when these particulars are duly impressed upon the mind we can be at but little loss to comprehend the true principles on which such cases should be treated.

Thus it is obvious that our first endeavour should be to relieve the system of the surplus water which pervades the tissues, more especially the cellular tissue; and it is also obvious that, in consequence of the great loss of albumen, this object should be effected with as little additional loss of that principle as possible. This purpose is commonly sought to be attained by the free use of diuretics, and these sometimes of a very irritating character. Now nothing can be worse than this practice as a system, for it is one fraught with danger to the structure of the kidney itself, and one which, by increasing the elimination of the albumen, contributes to the reduction of that strength which is already disappearing with such rapidity. Of course diuretics are of great value in the treatment of some cases of Bright's disease, but then they must not be indiscriminately used, and should not be relied upon as the chief means employed for the removal of the dropsy.

Another means whereby an endeavour is made to disperse the dropsy is by purgatives. These are also objectionable, though less so than the use of diuretics, since by them something more than water is abstracted, and they tend consequently to increase the weakness. Valuable, therefore, as purgatives are in many cases, they likewise should not be too much relied upon for the removal of the effused water.

The means of all others which should be resorted to is the frequent use of the vapour-bath. This is both powerful and safe, and admirably adapted to the object in view. A vast surface is brought into action, and little besides water is eliminated. The effects of the vapour-bath are usually most marked, and so grateful to the patient, that, as was particularly the case with Edward G., he often asked to be allowed to have it repeated at shorter intervals than had been ordered.

Next, remembering the enormous drain of albumen from the system, and the consequent impoverishment of the blood and weakness thus produced, it is obviously most desirable that this loss should, as far as possible, be replaced. The best idea that can be given of the extent of the drain upon the system through the loss of albumen is furnished by reference to the quantity of blood represented by the amount of albumen daily discharged. In the case of Francis S., the loss of dried albumen when his malady was at its height amounted to 70·4 grains in the twenty-four hours: being the quantity contained in 1000 grains or $2\frac{1}{4}$ ounces of blood. In Edward G.'s case the mean daily loss of dried albumen was in the first series of analyses found to be 310·8 grains, equal to 4400 grains or 10 ounces of blood. Now supposing this drain to continue daily, in forty-seven days and seven hours he would have lost a quantity of albumen equal to that of the whole of the blood contained in the body. The diet therefore should be highly albuminous, and should consist of eggs (especially the white portions) uncooked and of plenty of juicy and underdone meat. Unfortunately our wishes in this respect are often frustrated by the want of appetite of the patient: thus while Francis S. took freely all the eggs and meat ordered, Edward G. did not consume nearly the quantities of meat apportioned to him.

The third indication which must be fulfilled to still further improve the condition of the blood consists in the administration of such tonic remedies as act especially on the red corpuscles, as some of the preparations of iron. This indication may probably be promoted by the oxygenation of the system as far as possible by pure air and the use of an aqueous saturated solution of oxygen.

Lastly, we must endeavour to reduce the amount of albumen

discharged by the administration of appropriate astringent remedies, such as gallic acid.

Of course in some cases there are other indications which require to be attended to, and which may possibly arise hereafter in that of Edward G. Thus coma or convulsions may supervene. Upon the consideration of the appropriate treatment for these conditions it is not necessary that I should at present enter, as my object is to dwell chiefly on the treatment of Bright's disease only so far as this is elucidated by the two cases the histories of which I have given in the foregoing remarks. We should not forget, however, that Edward G's. is a case of fatty degeneration, and that therefore his diet has been regulated accordingly, and fatty and oily matters prohibited.—*Lancet*, *Dec.* 17, 1864, *p.* 684.

33.—ON THE ACTION OF TINCTURE OF PERCHLORIDE OF IRON IN THE CURE OF RENAL AND URINARY AFFECTIONS.

By Dr. Arthur Hill Hassall, Senior Physician to the Royal Free Hospital.

There are few remedies more frequently prescribed in the treatment of renal and urinary affections than is the tincture of perchloride of iron, formerly called muriated tincture of iron. The value of that remedy in a variety of such cases is undoubted, as in the albuminuria of Bright's disease, in hemorrhage from the kidney, bladder, or urethra, in spasmodic stricture, &c.

The tincture of the perchloride of iron consists of two atoms of iron in combination with three of chlorine, dissolved in water to which rectified spirit has been added; and it possesses the properties of an astringent, tonic, and styptic, coagulating blood or albumen with which it is brought into contact, and constringing the vessels and tissues to which it is applied.

Now, this astringent property is just that which a remedy ought to possess to be useful in the cases above referred to, and by it, it is usually supposed, it exerts its beneficial action; and certainly nothing would appear to be more plausible and more natural than this explanation.

Having, after the administration of this remedy, repeatedly tested the urine for the purpose of detecting in it the presence of iron, and having failed to discover the faintest trace of the metal, I was led to doubt the correctness of this view, and was induced to institute some experiments, in order to put the matter to the proof.

To a patient, T. L., labouring under an habitual urinary discharge, I administered for the period of more than a week a drachm of the tincture thrice daily; but, although I tested the urine on several occasions, in no instance could I detect the smallest trace of iron, notwithstanding that a pint of the urine was evaporated to a small bulk before being examined.

To a patient now in the Royal Free Hospital, Charles S., who is suffering from an extravasation of blood, three drachms of the tincture were administered on two consecutive days. The whole of the urine passed in the twenty-four hours of each day was collected, a pint of each sample evaporated to a small bulk, and tested as before, but with a similar negative result.

Lastly, I myself took in the course of a day three drachms of the tincture; the urine passed in the twenty-four hours being collected and analyzed, not only on the day on which the medicine was taken, but on the preceding and succeeding days. Still no iron was found.

I could enumerate several other instances in which iron had been taken and the urine analyzed, without even traces of the metal being subsequently discovered. The examples, however, I have quoted are sufficient to show that the tincture of perchloride of iron does not produce its beneficial effects, as generally supposed, in restraining the amount of albumen or of blood discharged from the kidney or other portion of the genito-urinary mucous track by coming in contact with the seat of the lesion and by its action as an astringent.

How, then, does this remedy act? That much of the iron contained in the sesquichloride does not find its way into the circulation at all, but escapes from the system with the undigested portions of the food, is certain; the black discoloration of the fæces under the use of this tincture, and indeed, I believe, under all the preparations of iron, is well known, the colour being due to a combination of the iron with a portion of the sulphur of the food—sulphuret of iron being thus formed. It might therefore be very plausibly presumed that while the greater part of the iron is thus thrown off by the bowels without having been absorbed at all, the hydrochloric acid, being set free, enters the circulation, is eliminated by the kidneys, and so comes in contact with the seat of the lesion; and that it is to the acid, and not to the iron, that the benefit is to be attributed. But if this view be correct, it is capable of being substantiated by experiment; and with this object I administered to two persons drachm doses, repeated thrice daily, of the perchloride; the urine of the twenty-four hours being collected and analyzed before, during, and after the administration of the ferruginous preparation. The results will be seen in the following tables :—

C. S——,

	Nov. 26th. Before.	27th. Before.	28th. During.	29th. During.
Quantity	62½ oz	70½ oz.	49 oz.	46 oz.
Acidity	30·35	30·18	33 gr.	30·14
Chlorine	115·0	137·0	90·0	140·0

A. H——.

	Nov. 17th. Before.	18th. During.	19th. After.	20th. After.
Quantity	56 oz.	57 oz.	33 oz.	53 oz.
Reaction	Faintly alkaline.	Faintly alkaline.	Faintly alkaline.	Faintly alkaline
Chlorine	95·0	84·57	76·0	44·98

The above figures show, 1st, that there was no increase in the acidity of the urine consequent upon taking the remedy; 2nd, that there was no increase of chlorine, and that therefore the hydrochloric acid of the perchloride was not eliminated by the kidneys either in the free or combined state: thus proving that the second view mentioned of the action of the remedy is also entirely unfounded.

These results are not a little remarkable; and we have, therefore, still to enquire, in what way does this medicine act in the cure of disease? Its effects are too rapid to allow it to be supposed that its operation is due to its influence in improving the condition of the blood by its action on the red corpuscles. We appear, therefore, driven to the conclusion that the perchloride of iron acts by its stimulating influence on the nervous system.

These observations are interesting, not alone as concerns this one preparation of iron; they also probably apply more or less to most of the other medicinal preparations of that metal, since it is at least certain that by far the greater part of the iron contained in them is not absorbed but escapes from the system by the bowels like that of the perchloride.

The particulars herein recorded are suggestive of further experiments calculated to throw additional light upon the subject, and which hereafter I may have the opportunity of instituting.—*Lancet, Dec.* 31, 1864, *p.* 740.

34.—ON DIABETES AND ITS TREATMENT.

By Dr. H. Bence Jones, A.M., F.R.S.

Each grain of starch that is taken as food, when acted on by the saliva or pancreatic fluid, is converted into sugar and becomes a source of fuel or power and ultimately passes out of the body as carbonic acid and water.

In diabetes the starch is still changed into sugar; but in the circulation and in the extravascular or parenchymatous structures the further change is partly or wholly stopped; more or less of the sugar remains as sugar, part reaches the kidneys and is thrown out in the urine, part exists in the structures unchanged; and thus the system is deprived of one part of that

conversion of latent into active force on which the nutrition and power of the organs depends.

In the body or out of the body, when once a series of chemical action is set up, the series tends to become continuous; it propagates itself, unless a change of conditions occurs. For example, grape juice will remain as grape juice, if kept from the action of other substances. Under well-known conditions it will ferment, and continue fermenting, as long as sugar, and ferment, and the proper temperature are present. If the conditions are are slightly varied, variations in the products will be observed. The series of actions will not pass rapidly from grape juice to alcohol, to vinegar, or to putrefaction; but according to the conditions one action or another action will be set up or will continue as long as that action is possible.

In health and in diabetes the sugar is subjected to different conditions. In health the chemical changes are propagated and continued to their full extent; whilst in diabetes the conditions necessary for perfect change are wholly or partly wanting. These conditions, on which the changes depend, are chiefly three. 1st. The proper temperature; 2nd. The presence of a not immoderate quantity of sugar; 3rd. The presence and activity of a ferment.

On each of these conditions a few words are necessary.

First, the internal temperature of the body, under all circumstances, is subject to so small a variation, that at present there is no proof that such a reduction can occur as can check the chemical actions in the human body, though under certain circumstances small animals can be exposed to cold so that sugar may be made to appear in the urine.—*(Proceedings of the Royal Society, December* 15, 1864.)

Secondly, by the most careful investigation, I have satisfied myself that Professor Brücke is right in saying that healthy urine contains sugar—that is, the amount of sugar derived from the food, or from the changes going on in the textures, is at all times more than can be entirely changed in the circulation and in the tissues. If, then, a considerable amount more is thrown into the circulation, some of it will pass out in the urine. Thus an injection of sugar into the rectum when absorbed causes temporary diabetes; so, also, large quantities of sugar, fruit, and farinaceous food tend to produce diabetes. Thus, also, if Bernard's amyloid or glycogenic substance were produced in excess in the liver, diabetes would result.

Thirdly, the ferment, the prime cause of change, has not been yet insulated. Whether it be the albuminous substance of the saliva or pancreatic fluid, or some other of the many albuminous substances in the blood, it requires for its action heat, the

presence of alkali, and an undisturbed circulation of the changing fluid.

With regard to the effect of alkalies, something will be said when I speak of the treatment of diabetes; but concerning the influence of unobstructed chemical action some remarkable physiological experiments must here be mentioned, because they not only furnish the most striking illustrations of the relations of mechanical to chemical disease, but because they also help to account for the effect of sudden mental shocks and nervous disturbances, which do so much harm to the diabetic patient.

Bernard made the great discovery that the mechanical injury of the floor of the fourth ventricle alters the chemical actions going on in the body so as to cause sugar in excess to appear in the urine. This is a most remarkable demonstration of the relation which subsists between mechanical and chemical actions in the living body.

M. Schiff, in his "Untersuchung uber Zuckerbildung" (1859) states that this diabetes results from injury of the vaso-motor nerves of the abdominal organs which arise from the thalami optici and the crura cerebri—*partent des couches optique et des pedoncules cérébraux;* and he also shows that by injury of other parts of the nervous system temporary or permanent diabetes is produced.

Thus cutting the posterior roots of the nerves arising from the cervical portion of the cord, leaving the anterior roots untouched, causes temporary appearance of sugar (irritative diabetes): whilst by division of the anterior roots on a level with or above the fourth cervical vertebra, permanent (or paralytic) diabetes results. These injuries, he says, cause an irritation or a paralysis of the vaso-motor nerves. The vessels of the liver become dilated and distended; and as a consequence there is an excessive secretion of sugar by the liver.

The permanent diabetes may be thus occasioned: but it is far more probable that the temporary diabetes is owing to the mechanical injury of the nervous system so affecting the vessels that the chemical changes are interrupted. The same phenomenon is seen in every fermentation. The chemical action is temporarily stopped when the fermenting fluid is disturbed by any sudden change.

These conditions of change may in diabetes be affected singly or all simultaneously. When the affection reaches to the extent of stopping the chemical change in the vegetable or animal sugar in the body, diabetes, intermittent or permanent, is the result.

That the disease is only a little way distant from health is shown by the existence of sugar in the urine in the healthy

state; so that a small quantity of sugar in the urine is no proof of disease; and here, as elsewhere, there is no defined limit where health ends and disease begins.

Diabetes is the arrest of a healthy state; an increased quantity of sugar appearing in the urine, because the actions that constitute health are stopped.

In perhaps half the cases of diabetes the arrest of change in the food-sugar constitutes the complaint. This is proved by the fact that when a strict antifarinaceous diet is observed the abnormal amount of sugar ceases, and the patient is well as long as he keeps to the strict diet. When this fact was published in 1806 by Dupuytren and Thenard, they used these words:—"Que le traitment qui consiste surtout dans un régime purement animal a le même degré d'efficacité que le quinquina dans les fièvres intermittants."—(*Annales de Chemie, vol.* 59, p. 45.)

But it was soon found that cases occurred in which a strictly antifarinaceous diet, consisting of animal food and water, did not stop the sugar in the urine. For days and weeks not a grain of vegetable starch or sugar may be taken as food, and yet sugar in excess will exist in the urine. Whence does this sugar come? It must either be taken in the animal food or it must be produced in the body.

Previous to Bernard's discoveries, this production of sugar when no vegetable food was taken admitted of no explanation. Now, the discovery of the amyloid substance in the liver, and of inosite in the muscles, lungs, brain, and other organs, gives a full solution of the difficulty. In the chemical changes going on in the body, animal starch is formed in the liver, and in other organs as in the prostate gland, and sugar in the muscles; not, as in vegetables, by the fixation of carbon and the decomposition of water by the action of light, but more probably by the gradual splitting up of the higher organic compounds of the different organs whilst performing their functions.

The good effect of an antifarinaceous diet in some cases, and its want of effect in others, marks the two great divisions of the disease—the two stages of diabetes. In the first stage vegetable sugar alone ceases to go through the healthy chemical changes, whilst the animal sugar is entirely changed; in the second stage, animal sugar as well as vegetable sugar are more or less unchanged. It may be concluded that animal sugar is more readily changed than vegetable sugar; and when in diabetes an antifarinaceous diet has no marked effect, then the conditions of change of the sugar in the body are furthest from the healthy state.

There is sufficient evidence that diabetes does not always progress from one stage to the other. The disease may halt anywhere in its progress, and remain stationary for years, or get

better or worse. A gentleman has consulted me for the last four years occasionally; he is now fifty-seven. When I last saw him the specific gravity of the urine was 1020, about three pints in twenty-four hours, containing about four grains of sugar to the ounce. He had been diabetic for twenty-nine years, during which time he married, and has now healthy children grown up. When he takes care about his diet, the sugar and symptoms vanish, but when careless, the symptoms return, and the sugar can be found in the urine.

Diabetes, then, in its mildest or first form, is the loss of power to change the sugar of the food; in its more advanced or more intense form, it is the loss of power to change the sugar produced in the organs and textures of the body, as well as in the food. The changes in the animal as well as vegetable sugar are arrested.

[Almost every remedy in the Pharmacopœia has been tried with the view of arresting the excretion of sugar, but none has any constant effect. Meanwhile, the effect of diet is far beyond that of any known remedy.]

An antifarinaceous, or, in other words, an antisaccharine diet will remove the sugar from the urine, and stop all the symptoms of the complaint in all those cases in which the power of consuming the animal sugar remains unaffected.

Even when the consumption of the animal sugar is imperfect or impossible, an antisaccharine diet will lessen the thirst, the flow of water, the dryness of the mouth, and even the constipation, and check, though it may not stop, the waste.

The simplest formula for the diet may be thus stated. All animal produce, including fish, flesh, fowl, game, eggs, cream, and meat soup should be taken; and all vegetable food that contains starch, dextrin, and sugar should be avoided.

As generally it is of the utmost importance to shun the forbidden food, I shall dwell upon it first.

The vegetable substances that contain most starch, dextrin, and sugar are rice, maize, arrowroot, sago, potatoes, oatmeal, peas, beans, bread, biscuit, toast, maccaroni, vermicelli, and all confectionery.

Fruits are even worse than vegetables. Apricots, plums, peaches, cherries, pears, gooseberries, are nearly as bad, and some worse, than rice and maize. Stout, porter, and ale, cider, port, Madeira, champagne, and sherry are more or less highly saccharine; cocoa and chocolate contain near 20 per cent. of starch and dextrin naturally, and more is often added.

The harm of each of these substances may be determined by the amount of starch, dextrin, and sugar they contain.

The following table, in which the fruits and farinaceous vege-

tables are taken as perfectly dry, will answer many questions regarding the diet of a diabetic patient:—

	Amount of Starch, Dextrin, or Sugar.
Ripe dry Apricots	about 93 per cent.
,, Plums	,, 92 ,,
,, Peaches	,, 86 ,,
,, Cherries	,, 85 ,,
,, Pears	,, 84 ,,
,, Figs	,, 79 ,,
,, Gooseberries	,, 37 ,,
Dry Rice	,, 90 ,,
,, Maize	,, 88 ,,
,, Arrowroot	,, 77 ,,
,, Potatoes	,, 76 ,,
,, Oatmeal	,, 70 ,,
,, Peas	,, 67 ,,
,, Beans	,, 67 ,,
,, Bread	,, 61 ,,
,, Milk	,, 21 ,,

Stout	about 45 to 64 grs.	of sugar per ounce
Porter	,, 23 ,, 40	,, ,,
Ale	,, 12 ,, 45	,, ,,
Sweet cider	,, 18 ,, 44	,, ,,
Port	,, 16 ,, 34	,, ,,
Madeira	,, 6 ,, 66	,, ,,
Champagne	,, 6 ,, 28	,, ,,
Sherry	,, 0 ,, 12	,, ,,

If dry rice contains 90 per cent. of starch, dextrin, and sugar, and potatoes contain 76 per cent. of starch and dextrin, and if all the starch and dextrin pass off in the urine as sugar, it is evident that to forbid potatoes and to allow instead an equal quantity of rice is simply ordering the quantity of sugar from this source in the urine to be increased 14 per cent., thus adding to the thirst and waste. Or if half-a-pint of port wine is forbidden containing from 128 to 272 grs. of sugar, and if a pint of porter or stout is ordered which contains from 368 to 960 grs. of sugar, it is clear that the quantity of sugar in the urine will thus be increased from half-an-ounce to an ounce and a-half daily.

Before passing to the best diabetic diet, there are two substances—bread and milk—which require to be further mentioned here.

Ordinary bread contains water, salts, starch, dextrin, sugar, and gluten.

If the salts, starch, dextrin, and sugar are washed away, the

gluten remains, which, in a chemical point of view, is as unobjectionable as meat.

In making the different kinds of gluten bread this washing is more or less perfectly performed.

In the following analysis the water, starch, dextrin, and sugar were determined, the residue or difference was taken as gluten :—

	Water.	Starch and Dextrin.	Sugar.	Gluten.	
Ordinary bread .	36	49	1	23	per cent.
Aerated bread .	37	42	2	19	,,
Gluten bread from Toulouse . .	2	16 to 44	0	82 to 54	,,
Dried bread .	2	60	1	37	,,

The best washed gluten bread contains less starch than bran cakes or any brown bread ; Dr. Pavey's almond bread is free from all starch, but the almond flour must be well washed to remove the sugar and dextrin, of which 10 per cent. are present.

Badly-washed gluten may be made into dry bread containing bulk for bulk more starch than ordinary undried bread ; thus an excess of gluten bread may keep up the amount of sugar in the urine, and prevent an improvement in the symptoms.

With regard to milk, one hundred parts may be taken to contain three parts of lactine, or about half-an-ounce of lactine exists in a pint of milk. If all of this animal sugar was incapable of being consumed in the system, milk would be nearly as injurious as an equal quantity of many wines, and the best sweet ale ; but experiment shows that this sugar is often partly or entirely consumed.

A diabetic patient lived upon butchers' meat alone for two days. The quantity of urine passed was forty ounces the first, and forty-two ounces the second day ; specific gravity, 1029·0. No trace of sugar could be found. He then took milk for two days ; the first day eighty-eight ounces, the second day ninety-nine ounces. The urine was forty-five and a-half ounces the first day ; specific gravity, 1024·1 : and the second day sixty-nine ounces ; specific gravity, 1011·9. Sixty-seven grains only of sugar were passed the first day, and twenty-three the second day.

At a more advanced period of the disease, when strictly animal diet did not cause the sugar to disappear from the urine, milk alone was again taken.

The first day 138 ounces of milk were drank ; the urine was 61 ounces ; specific gravity, 1030·5. The second day 88 ounces of milk were taken ; the urine was 34½ ounces ; specific gravity, 1027·8. The quantity of sugar in the urine was 854 grains the first day, and 414 grains the second day. At this time, on

animal diet alone, one day 280 grains of sugar were passed, and the next day 600 grains.

Thus milk is more or less injurious according to the stage of the complaint. When animal sugar can be consumed milk is comparatively harmless.

In the advanced stages of the complaint curds or sour milk, with the acid nearly neutralised by potass, soda, or ammonia, would be as unobjectionable as gluten bread.

There are two great ends to be gained by the use of medicines in diabetes.

Of these the first and most important is to promote the oxidation of the sugar; or, failing this, to compensate the system for the loss of saccharine fuel, and the consequent loss of power and nutrition by promoting the supply and oxidation of the oleaginous fuel.

Of all the medicines that can be given for the promotion of the oxidation, whether as sugar or fat, in the body, iron and alkalies are the most energetic; and hence, beyond all other remedies, iron or ammonio-citrate of iron with excess of ammonia, or with other alkalies, are the best medicines for diabetes. The iron may be given in potass or Vichy or in Fachingen water, and that preparation which confines the bowels least is most to be preferred. Hence the potassio-tartrate and Griffiths' mixture are often useful.

Alkalies without iron promote oxidation. This is very evident in the copper test for sugar. M. Miahle even has stated that alkalies are the specific for diabetes; without doubt they are of importance in promoting oxidation. Soda or potass may be given in the caustic state or as carbonates. Carbonate of ammonia in ten, fifteen, or even twenty-grain doses thrice daily in any gaseous mineral lessens the thirst.

Long since Professor Graham tried the phosphate of soda, with three equivalents of soda, on the ground that the blood required this substance and could not get it in the animal food. I have not found any great advantage from its use.

Vichy water, more particularly the Celestin, is recommended by M. Bouchardat. It contains between eighty and ninety grains of carbonated alkali and alkaline earths in a quart of water. The Hospital spring contains about ninety-six grains, the Grand Grille ninety-two, the Hautrive eighty-nine, and the Celestin eighty-five grains to a quart of water.

Carlsbad Sprudel water and the more aperient Muhl spring is highly praised by Dr. J. Seegen. It contains about one-fourth or less of the alkali of Vichy water, but half a drachm or more of sulphate of soda in each quart gives an aperient action which the common salt of Vichy water rarely possesses. Marienbad Kreutzbrun water is twice as aperient, but rather

less alkaline than Carlsbad Sprudel. Fachingen water has one-third or more of the alkaline power of Vichy. Seltzer water one-sixth.

Besides alkalies some animal substances are thought to promote change in the sugar in diabetes. Of these rennet and pepsine may be mentioned. In 1852 Dr. James Gray published some remarkable results in the 'Edinburgh Monthly Medical Journal,' p. 396, but I am not satisfied that rennet is very useful in diabetes. It is an albuminous substance in a state of change, and, therefore, it exactly fulfils the conditions required in the undiscovered specific for diabetes. It should be well washed with water to remove the adhering sugar and dextrin, and it should be given on an empty stomach, so as to enable it, if possible, to act on the sugar in the blood rather than on the sugar in the food.

Pepsine is another albuminous substance in a state of change. Its action in the stomach is to help the solution of the albuminous food, but when it passes into the blood it might be the animal diastase that carries on the change in the sugar. With this idea it has been given in diabetes, but with no satisfactory result; although many patients have said they were better whilst taking this remedy, yet I have never found the sugar diminish under its use. It usually helps the action of the bowels.

Lately oxygen gas has been tried by Dr. Richardson in diabetes. It would be splendidly simple if this were the specific for diabetes. This view implies that a deficiency of oxygen is the cause of diabetes, but the chemistry of the disease does not admit of this explanation. There must be a much more complex chemical error than a want of oxygen, and it is far more probable that no deficiency of oxygen exists, but that it does not act because the proper animal diastase is not present.

Vegetable and animal oils and fats constitute important remedies in diabetes. Of all these, cod-liver oil and cream are most frequently used. The following case may be taken as an instance of the amount of cod-liver oil that can be given :—

A man, aged twenty-four years, was admitted into St. George's Hospital, having lost two stone in weight during eight months. He passed seven quarts of urine daily. He remained under treatment for a month, during which time he was on animal diet and cod-liver oil. He began with half an ounce daily, and this was gradually increased up to eight ounces. The quantity of urine fell to two pints and a-half, specific gravity 1030, and he increased in weight from 8 st. 8 lb. to 9st. 11b.

Cream may be given in any quantity until the tongue begins

to be coated, then it soon disagrees, and the stomach refuses to take it, or rejects it when taken.

Fats, combined with alkalies, as soaps, are more ready to undergo oxidation than when the glycerine is unseparated. Pure glycerine is, however, often very useful as a substitute for sugar in tea and in other liquids.

To lessen the thirst and the craving for food opium is very useful. The alkaloids of opium diffusing out of the blood in contact with the nerves of the blood-vessels cause contraction of the capillaries; this affects secretion, so that the quantity of urine, saliva, bile, and intestinal secretion is greatly diminished. The drain of urine and the desire for food are thus checked, but the increased constipation and dryness of the mouth almost, and in some instances quite, counterbalance the gain obtained by checking the flow of water. By the use of very small quantities of opium, as five or ten grains of Dover's powder, or five or ten drops of laudanum once or twice daily, the thirst and excessive flow of urine may be stopped, and the constipation may not be excessively increased.

The second great object in the treatment of diabetes is to remove the constipation.

The excessive determination of water to the kidneys causes increased dryness elsewhere, hence the mucous membrane and the skin are harsh and dry; probably also the production of acid being greatly diminished by arrest of change in the sugar, the healthy secretion by the intestines and skin cannot take place.

Notwithstanding the amount of food eaten, the action of the bowels usually is very difficult. All saline aperients increase the thirst and pass off by the urine. Magnesia from the absence of acidity is usually inactive. Castor oil is by far the best aperient, when it does not nauseate, then capsules containing castor oil with minute quantities of croton oil are most efficacious. Compound extract of colocynth with jalapine, scammony, or gamboge, or podophilline will act when oil cannot be taken. Mercury when soluble combines with the albuminous matters with which it comes in contact and sets up increased chemical action. The nutrition of the parts to which it is directly applied, or carried by absorption and diffusion, is so altered that increased action, inflammation, and ulceration are produced. This altered nutrition has no influence on the oxidation of the sugar or fat, and hence diabetes is not benefited by mercury. Calomel may be used as an aperient, but it has not any advantage over other chemical or mechanical irritants to the mucous membrane of the bowels, and promoting a more rapid change in the albuminous structures of the textures causes a feebler nutrition to take place.

In extreme cases the constipation of the bowels becomes the most serious symptom. The chemical disease leads to a mechanical difficulty amounting almost to an obstruction of the bowels. Strong chemical irritants are required to excite the muscular action, sometimes even "croton oil" is necessary. When the bowels do act the prostration of the strength is sometimes alarming: so little power seems to be set free in the body that any extra expenditure seems to deprive the heart of the force necessary to carry out the circulation, in the same way as a fatiguing journey to London for advice in extreme mental anxiety will bring a prostration of strength from which with difficulty recovery takes place.—*Med. Times and Gazette, Jan.* 21, 28, *and Feb.* 4, 1865, *pp.* 55. 81, 109.

SURGERY.

DISEASES OF THE BONES AND JOINTS, ETC.

35.—ON THE IMMEDIATE TREATMENT OF FRACTURE BY FIXED APPARATUS.

By Jolliffe Tufnell, Esq., Surgeon to the City of Dublin Hospital.

[Mr. Tufnell gives a table showing the practice in ninety of the principal hospitals in Great Britain. He constructed it from information supplied by the house surgeons of the respective places. An analysis of this table gives the following result.]

The various materials employed for the purpose of making fixed apparatus for the treatment of fracture of the leg are—starch—starch combined with pasteboard or gutta-percha—gum and chalk—plaster of Paris—plaster of Paris and gum—gum only—dextrine and glue; and the relative degree of frequency with which each one is employed will be exemplified by the following detail:—

Starch, with or without side splints, in	57	Hospitals
Gum and chalk, in	18	,,
Plaster of Paris, alone, in	6	,,
Plaster of Paris and Gum, in	1	,,
Gum, only, in	4	,,
Dextrine, in	2	,,
Glue, in	2	,,
Total,	— 90	

In this long list of hospitals the period usually adopted for the employment of the starch or other fixed material has been stated in the majority of cases; and, yet in seven instances only out of the whole number do I find the custom of early putting up of the fracture by it resorted to. Good reasons must necessarily exist for this line of practice to be so general and uniform, and the danger or risk from constriction of the limb by the starch bandage, as ordinarily employed, must, I consider, be regarded as the cause of its primary rejection, and employment only after swelling has subsided, or partial union of the bones taken place. I have, myself, for many years in hospital and private practice, but especially in the latter, been in the habit of using a modification of the fixed apparatus, such as is not, I

believe, described in any work, or otherwise generally known; for, out of all the replies which I have received, I find allusion to it made only by one gentleman, viz., Dr. Turner, Senior House Surgeon of Guy's. I purpose, therefore, briefly alluding to the mode of procedure here; and, I trust that I shall be enabled to make the plan of action intelligible to all. The material required for forming these splints is very generally available, consisting only of lint or old linen for the inner lining; strips of the same, torn three inches in width, and long enough to reach from the head of the tibia to the sole of the foot. To make the splint itself:—The white of eight eggs and half-a-pound of flour for the fixing substance. These are all that are required; each and every one of which is to be procured in almost the poorest home—and as such, this splint is readily available to the surgeon in country practice.

The main feature of difference between this mode of setting fractures and every other kind of fixed apparatus, is the construction of the splint in two halves, and the applying of the bandage, which is to form the same lengthwise, instead of circularly, thus avoiding all possible sources of constriction of the limb.

Supposing, then, that the fracture has occurred in a city, or wherever else the most desirable materials are to be procured, the surgeon sends for the following, and places them beside him before interfering with the patient, further than to strip him of his clothes and lay him upon a properly prepared bed, upon the side opposite to the fractured limb—the leg itself being supported easily on a pillow—and the fracture, as far as possible, reduced. The articles required are—a table for spreading the bandages upon, a wash-hand and small sharp-edged basin, eight eggs, and half-a-pound of flour, as before stated; a large iron or silver spoon, a large knife, three calico roller bandages, half-a-yard of Taylor's lint, and some hot water. The lint is first thrown upon the limb, from the knee to the sole of the foot, and cut roughly into the outline of the limb, of a size sufficient to tuck under the sole at the bottom, and from side to side of the leg.

This lint is placed in the large basin, and hot water poured upon it, so as to saturate it completely, and whilst dripping, and without being wrung, it is lifted out and put upon the outer side of the limb of the patient who is lying with the leg bent, and exactly in the same position as recommended by Pott, with this difference that the limb is on the inner instead of its outer side, by this measure insuring subsequent inversion instead of eversion of the foot. Extension and counter-extension are now made by the hands of two assistants, and the fracture set by the surgeon, who models the wet lint on to the limb with

his hands, so that it is as closely applied as a stocking would be; any starting or spasm of the muscles is prevented by the assistants just laying their hands steadily upon the limb. The surgeon now breaks the eggs upon the sharp edge of the small basin, drops the yolks into it, and the albumen into the other, having first thrown out the hot water which was previously in it for wetting the lint; he then takes sufficient flour, according to the size of the eggs, and with the spoon beats it and the albumen into a homogeneous mass of the consistence of pancake. He next measures with the end of a roller the length of the limb, from the knee to the centre of the sole, and placing the forefinger of his left hand, pointing upwards, and the forefinger of an assistant opposite to him at the required distance, he winds the whole of the three roller bandages, one after the other, around the fingers, and then with a scissors divides them at either end; thus in a few moments obtaining the strips necessary for the splint, all of exactly the same and the proper length. Each of these tails is now spread thickly with the egg and flour upon one side, and as spread, folded double, with the plastered surfaces opposed and laid aside. The object of this is to keep them moist, and in hot weather from drying; as soon as they are all done the surgeon commences, and having them handed to him one by one, he lays them on the limb, moulding them to it, layer after layer, from the innermost portion of the tibia within to the farthest point of the calf of the leg that can be reached behind, covering accurately the instep, the toes, sole, and heel, and not leaving a wrinkle or crease. The large knife which has previously been used for spreading the bandages with egg and flour, is now employed as a spatula, and taking a few of the tails of bandages which have been left unspread, these are passed beneath the limb, and one after the other brought around it so as to confine the wet tails to the leg, and not allow them to be raised from off it as they otherwise would be by the expansion of the heated air which becomes rarified within.

The limb is now *left uncovered* till next day; if covered the splint will not dry, but retains a kind of pasty condition, whilst if exposed to the air for twenty-four hours it will be as firm and as light as cardboard on the limb. Upon the succeeding day the patient turns over upon his opposite hip, and the same proceeding is repeated exactly as before, taking care that the lint lining goes well over the limb, so as to be in advance everywhere of the plastered strips, which if allowed to come in contact with the other portion of the splint would intimately adhere to it, and cause difficulty in separation of the halves; this too is allowed to dry. Upon the succeeding morning the two half splints, or only the second half, as the surgeon may think fit, are removed, the edges trimmed neatly

with scissors, the second half overlapping the other by at least half-an-inch, ensuring correctness, whilst all pinching of the integument is prevented; when the roller bandage is subsequently put on, the two portions of the splint, in fact, should glide, as it were, the one over the other. Nothing more remains to be done; a couple of fillets to confine them to the limb, or a lightly applied roller bandage are sufficient, and the patient may now lie in bed with his leg straight or bent, as he wishes, raised up upon a pillow, or slung, whichever way he fancies or feels it easiest; and after a few days may get up, supporting the extremity by a sling round his neck.

Subsequently, when the patients come to exercise and move about, and the size of the limb to decrease, the splints may be brushed over inside with hot paste (as also round the edges), and a piece of chamois leather, previously cut to the size and shape, moulded in to form a softer and additional lining.

These splints are very very light, weighing, when dry, but a few ounces, yet from their extreme accuracy in fitting the limb, and evenness of pressure, most surprisingly strong.

The same material I also employ in the immediate treatment of fracture of the fibula, and, at a later period, in fracture of the thigh, used in the circular form; put on, however, in tails, imbricated one over the other, and not as a roller bandage. Thus applied it is, of course, necessary to slit it up prior to removal, and for this purpose I employ a blunt gorget with the handle reversed, using it as an ordinary director, and cutting down upon the grooved steel with any sharp-pointed knife. Seutin's pliers and all other kinds of scissors and dividers I have tried and found objectionable in use, causing more or less anoyance and pain to the patient from the pressure exercised upon the soft parts in making the division of the harder external case, whereas the gorget slips along upon the skin, whilst its broad, round, and polished surface, is cut down upon without the slightest motion or pressure, and is not noticed hardly by the individual.

Such is the mode which I myself now employ, and such the form of proceeding in reference to fixed apparatus that I think will, upon experience, be found to be the most satisfactory to the practising surgeon; but, as so many other modes of putting up fractures in starch, gum and chalk, gum, &c., &c., have been referred to in the table of hospital practice here given, I would add a few words in reference to each, and my experience as to the advantages and disadvantage of the several plans.

The condition of one individual with a fracture of the leg, put up in a properly fitting and well applied fixed apparatus is so incomparably superior to that of another with a similar fracture, treated in any other kind of splint, that whatever can tend

to the removal of objections, needs, I feel assured, no apology. The sufferer with fracture of both bones of the leg, set in tin, wooden, or other ordinary splints, is, for a considerable time after the receipt of the injury, dependent, and unable to do anything for himself; the other is comparatively independent; and fractures treated in the way described, of the tibia only, of the fibula only, and both bones together, have turned out so satisfactory that I cannot but speak strongly in its favour. The union has been firmer, in the same number of weeks, than under any other plan. Why it should be so is evident from two reasons—firstly, the mobility of the fractured ends of the bone is reduced to nil, and the circulation of the limb is normally maintained—no circular constriction existing to produce venous engorgement, and interfere with arterial supply. The egg and flour splint, made as described, has this great advantage also, that whilst being considerably firmer and lighter than the starch or other material, as ordinarily applied, it requires no padding for the protection of osseous prominences and projections. In the common mode of applying a starch bandage every point must be well protected by tow or cotton, or some other soft defence, and the heel, tendo Achillis, or instep, the metatarsal bone of the little toe and the ball of the great, the spine of the tibia, its tuberosity, the head of the fibula, the patella, and condyles of the femur, all require padding to prevent ulceration, whilst the longitudinal splint receives its own indentations so accurately as to press unduly on no spot, and retains its position *in situ* so completely as to maintain its exact position when once applied, thus obviating or rather preventing the restlessness which accrues from pain caused by the pressure of a splint upon a projecting point of bone, and which so often irritates the patient and causes him to undo and loosen the fracture for the purpose of obtaining relief. Its stiffness upon drying, too, is such as to obviate the necessity of resorting to any supplemental support from pasteboard, gutta percha, whalebone, or any other of the stiffening matters usually employed as adjuncts to starch, and which all, more or less, press upon the limb. White of egg and flour, therefore, used as directed, are the substances that, in combination, seem to be most desirable in practice—they are everywhere obtainable, and at a very trifling cost.

Gum and chalk are the next best materials—indeed, as for stiffening purposes, they will equally answer as the former. They are daily becoming more and more used, and have superseded starch in very many places; whereas, in no instance have I found starch to have replaced gum and chalk, when once introduced.

The general mode of using this combination is to rub up

equal parts of powdered gum Arabic and prepared chalk, or one part of gum to two of chalk, with cold water, to the consistence of thick cream, or white paint, at the time of use, and at the setting of the fracture; whilst others keep the mucilage of gum Arabic ready made, and when wanted pour it upon finely powdered prepared chalk, and rub it up to the consistence desired. The general directions given for its employment I subjoin, and the special plans adopted at some of the hospitals also annex.

GUM AND CHALK.

Mode of Applying.—The mixture should be made of equal parts of prepared chalk and gum Arabic in powder, rubbed up together, with sufficient water to make it as thick as good cream, for unless made tolerably thick it will not answer well.

The limb is now to be evenly surrounded with wadding, or cotton wool, and then firmly bandaged with an ordinary roller from the toes to the knee. This bandage is then to be well smeared over with the mixture, and next a bandage, well soaked in the mixture and then rolled, is to be applied; this is again smeared over with the gum and chalk, and the whole left open to dry.

If it is desired to make the casing more than usually strong, another dry bandage is put over the last, which at every turn is smeared over with the mixture; when dry the bandage ought to look quite smooth and white.

This bandage looks better and keeps cleaner than if a simple dry bandage is placed externally. The bandages should be applied very evenly.

In saturating the bandage with gum and chalk, it is a good plan not only to soak the roller bandage in the mixture, but also to brush it into the bandage as it is rerolled, with a stiff shaving brush.

By immersion of the limb in warm water the whole can be removed if desired.

The following are the plans adopted in the undermentioned hospitals, viz.:—

Birmingham.—Equal parts of gum and chalk rubbed down into water, to a proper consistence.

Leicester.—A simple roller is first applied, then strips of stiff brown paper, about two inches wide, smeared with a solution of gum and chalk in equal parts, made to sufficient consistence, placed both longitudinally and circularly over the fractured parts, and outside these a roller bandage saturated with the same.

This bandage is used at once in recent simple fractures of one or both bones, if transverse, but not if oblique.

West Liverpool.—Put on a dry roller, then mould pasteboard side splints; a dry bandage is placed over these to fashion them to the limb. The gum and chalk (of the consistence of thick cream), is then well rubbed into the bandage with the hand, and allowed to dry.

If extra firmness be required, another bandage is applied over this, and treated in the same manner, the heel being covered with the bandage, as it prevents all complaints of soreness and existence of swelling.

Middlesex.—Cover the limb with a thick layer of cotton wool; over this a simple bandage, next a mixture of gum and chalk, with a few strips of bandage extra, smeared in the mixture, placed over the seat of fracture.

STARCH.

Starch is, of all the materials used for fixed apparatus, that most commonly employed; but only so, I consider, from the two previously-mentioned not having been tried. It is, in my opinion, far inferior to either gum and chalk, or white of egg and flour; but, as being in such general use, I append it with the modes of application in several institutions.

Aberdeen.—Soak a roller in boiled starch the thickness of common mucilage; apply it from the extremity up to prevent swelling, in the same manner as any ordinary bandage—two or three layers; next apply two softened pasteboard splints, one on each side, confined by a dry roller; these splints are to be kept on for a few days, then removed, and the fracture left in the starch only.

Birkenhead.—A common roller is first applied to the limb, then five or six pieces of pasteboard, from six to nine inches long, and one inch and a half in width, soaked in hot water, are to be accurately moulded to the limb, around the fractured part; a thin layer of cotton wool, or a piece of lint, being placed under the ends of the pasteboard to prevent any undue pressure and possible ulceration of the skin.

Over these strips of pasteboard a wet roller is placed, formed of cheese cloth or open canvas, its porous nature absorbing more readily, and in greater quantity, the starch, which is gently rubbed into its pores, the starch being as hot as the hand can bear it, and as stiff as it can be well used.

Canterbury.—Apply the starch bandage in strips, enclosing the heel; then a common roller, and again starch; then bandage, and leave it to dry.

Chester.—Well soak a bandage in thick starch; then roll up for use; apply the pasteboard splints one on either side, and bandage over all.

Dorset.—A bandage is unrolled and wetted, then soaked in warm starch, and applied as an ordinary roller.

Essex.—Pad the bony prominences with raw cotton; apply a common dry roller; next softened pasteboard splints on either side, and over all a well-starched roller bandage.

Glasgow.—Wrap the limb in a layer of cotton wadding, or a dry roller; then draw a bandage through starch, so as to saturate it; apply this as a roller over pastebard side splints, and a dry roller over all.

Hereford.—Bandage the limb and foot to above the knee; smear over with starch; pad the projections of the knee, tibia, and ankle, with tow; three wide strips of pasteboard, well softened in hot water, are to be applied; the posterior passing over the heel and terminating in the sole of the foot; the whole is then firmly bandaged and starched, a second layer being applied if necessary.

Portsmouth.—The bandage should be only two inches wide, making no reverse turns.

St. Bartholomew's.—First apply a flannel bandage to the leg next a dry calico roller, and then lay on the starch with a brush.

Staffordshire.—First put on a bandage wet; then rub this over with starch; next a calico roller soaked in starch; rub this well with starch on the surface, and leave to dry.

University College.—Apply a roller evenly as far as the knee; next two splints of coarse strong pasteboard, and shaped to the foot and leg, so as to grasp it, with their edges carefully bevelled —these having been first well soaked in hot water. Starch of medium consistence is rubbed on the outer side of these splints, which are then moulded to the limb by the hands of an assistant. Whilst the bandage is being applied starch is well rubbed into each turn of the roller, and afterwards plenty of starch is rubbed into the whole case.

If much strength is required a second bandage is similarly applied; when quite dry the case is cut open along the line of junction of the side splints, and, if necessary, the edges are pared, and then firmly brought together by a dry roller.

The bandage next the skin is dispensed with by Mr. Erichsen, who substitutes a small layer of cotton wool, so that there is less risk of strangulation taking place; and it also allows a fracture to be put up earlier than it could otherwise be done.

The objection to it is, that in very oblique fractures there is less certainty of obtaining an accurate coaptation of the parts.

Westminster.—Roll a bandage through a moderately thick solution of hot starch; let it stand an hour; then apply it to the leg; next smear a thick solution of starch evenly over, and allow it to dry. If not stiff enough to satisfy, another bandage is to be similarly placed over it when *quite dry*, but not before.

Chichester.—A dry roller; several splints of stout mill-board, softened in boiling water; then a bandage, thoroughly starched; and over this a dry roller.

Northampton.—First encase the limb in a thick layer of cotton; next a gutta percha splint, softened in boiling water, is to be moulded upon each side; a roller bandage is then to be put on in the ordinary way, rubbing starch in between each fold, made as thick as possible, and over the surface, so as to permeate it thoroughly.

PLASTER OF PARIS.

Plaster of Paris comes after gum and chalk in frequency of use. It is firm, and sets quickly; but is not so easily removed from the limb, and is very liable to crack. In using it the calico or other material employed for the bandage should be of very open texture, so that the plaster can be well worked into the meshes of the stuff, and not merely lie applied to its surface. It may be rubbed dry into the bandage, which is then rolled up, and wetted as it is applied to the limb; or the whole bandage be dipped entire into cold water, saturated, and then put on. Other surgeons use it thus:—They make a mixture, with cold water, of finely powdered good, dry, and *fresh* plaster of Paris, to the thickness of cream, in a basin; then unroll the bandage from a basin of cold water into the basin in which the plaster of Paris is contained; roll the bandage again in the solution of plaster of Paris, and apply it quickly to the limb. The names of some of the hospitals who adopt it, and the modes which they adopt, are subjoined.

Devon and Exeter.—First put a single dry roller bandage on the leg, and then another bandage previously rolled into the dry finely powdered plaster, well rubbed into it, and soaked in water for a short time before being applied; after being put on a moderate coating of wet plaster is to be smeared over all.

Lincoln.—The bandage, with the plaster well rubbed into its meshes, is to be applied to the limb dry, and then wetted through, more dry plaster being rubbed on the bandage, and more water; then a tolerably thick layer of plaster is to be put on, and allowed to dry.

Liverpool.—Dr. Armstrong, of the Northern Hospital, Liverpool, puts up oblique fractures of the tibia at once, as follows:—A competent assistant holding the limb in accurate position, the surgeon takes a piece of muslin long enough to reach from above the knee behind round the heel to the toes; and broad enough to enclose the limb, with the exception of an inch-and-a half over the line of the tibia.

Having first tested the plaster to see how long it is setting (and if longer than five minutes, hot water, or a little salt, must

be added), mix a good quantity of plaster, rather thin; soak the muslin in it, and apply it accurately to the limb, coating it well with the plaster of Paris behind and all over.

The assistant must hold the limb until the plaster sets. In this splint there is vent for any swelling; and, as the plaster is as firm as stone, no change of position can take place.

Compound fracture, when the wound is in front, can also be treated in this way; its cheapness is also a recommendation.

South Devon—The dry powder is firmly and carefully rubbed into a roller of proper texture, with open pores, and of sufficient length; this is then accurately applied to the limb, each turn of the bandage being damped by cold water; and, subsequently, a layer of the powder is sprinkled over all and moistened.

Taunton.—Mix the plaster in a basin, and pass the bandage through it; apply the roller, rubbing more plaster into the pores of the bandage.

GUM.

Gum and dextrine are but scantily employed, as compared with the substances already spoken of. Glue, too, is occasionally used. Gum seems to be generally rejected in consequence of the number of coatings necessary to give to the splint the required consistence. Dextrine, because of its being difficult to procure, and more expensive than the others; and glue from its being less clean in its application. The hospitals in which the latter is occasionally employed are St. Vincent's and Dr. Steevens' in Dublin, and by Dr. Pentland in the Infirmary at Drogheda. Gum, when resorted to, is thus directed to be used.

Bedford.—Envelope the limb in a layer of lint; then bandage firmly; next apply a thick solution of gum Arabic; allow it to dry, and then put on a second and third coat of gum solution.

Reading.—Place a layer of cotton wool round the ankle; then apply a washed bandage from the foot to the knee, and coat with gum; then take some pieces of lint, about two inches broad, doubled three times; gum them well, and place them on each side of the leg for two-thirds of its length; then put on another bandage from the foot to the knee, and gum it well and leave it to dry; the gum will have to be so applied about three times before it is hard enough.

St. George's.—A dry roller is first applied; then pasteboard splints; and, lastly, a gum bandage outside all.

St. Thomas's.—Three or four strips of lint, about an inch and a half broad, are dipped in thick mucilage of gum and applied longitudinally along the leg; over this a wet roller bandage is placed, and at each turn a little mucilage is rubbed in; over the lint a second bandage is placed to give sufficient strength.

DEXTRINE.

Charing Cross.—Put a roller bandage on the leg; then, with a brush, plaster on hot dextrine, only liquid enough to soak through the calico.

To sum up; the principal points in connexion with the treatment of a recent fracture by the fixed apparatus are:—

1st. To adopt longitudinal strips instead of a circular bandage.

2nd. To make the splints in two portions, excepting in cases of fracture of a single bone.

3rd. If making the splint in one piece, to do so by imbricating the bandage in tails, and not using it in the roller form, as the turns in doing so cannot but more or less constrict the limb; besides, grating of the fractured ends of the bones cannot be avoided in the rolling up of the limb.

4th. In no case should the circular form of fixed bandage be adopted when the surgeon cannot inspect the limb within a few hours of its application.

5th. If constriction follow, and the limb requires to be freed, the splint should be slit up upon a broad director, such as a paperknife, if better be not at hand, cutting down upon it with the point of a sharp penknife or bistoury.

6th. Never to use scissors or pliers when the last-mentioned mode can be adopted; and if any idea of using scissors be entertained then to employ the longitudinal tape recommended by Séutin when forming the splint.

7th. To use gum and chalk of the consistence of thick paint, or flour and white of egg of the firmness nearly of pancake, in preference to starch or gum, as drying more rapidly and being lighter, and not needing pasteboard or other additional support.

8th. If starch should be employed, to use it hot and as stiff as it can be worked.

9th. To pad well all bony projections when starch is used.

10th. To *tear* the pasteboard employed as side splints so as to bevel the edges, and not cut the splints out with a knife, in order to prevent the risk of ulceration, as laid down by Gamgee.

11th. To select pasteboard of thick and porous kind, which will readily absorb the starch and water, and be easily moulded to the limb.

12th. If the pasteboard at hand be too thin, then to use two or three layers, smearing each of them, and applying them together in two or three thicknesses.

13th. To use open-textured material for the bandages, more especially when plaster of Paris is employed.

14th. To see that the plaster of Paris is fresh, finely-powdered, and dry, and if so, employ cold water in its mixing; but, if the plaster is not fresh, then to use tepid or warm water.

15th. To steady the limb with side splints if necessary, or sand bags, for the first few hours after the fracture is set, or until the first half of the splint (if made in two portions) is dry.

19th. If an opening for the dressing of a wound (as in compound fracture, or ulcer in disease of the joints, or for the application of leeches), be required, then to form it in the method recommended by Dr. Browne, of Galway, or the next mentioned, and not in the ordinary way of roughly cutting the portion out.

"Dr. Browne's plan" is as follows, viz. :—To place a piece of the thinnest zinc, the exact size of the opening intended to be made, over the site of the wound, or required surface, and over this to form the splint. When dry and hard to cut down upon the centre of this piece of zinc, with a sharp knife ; and then, having come down upon it, to cut cautiously with a pair of scissors round and round, until the piece of zinc, which has acted as a shield to the soft parts, is set free. The next plan, or that adopted by myself, is to leave the opening in the formation of the splint thus :—After encasing the limb or joint with lint, to trace out, with the point of a pen and ink, a circle larger than the opening required ; then, as each layer of bandage comes over this spot, to cut out with sharp scissors the requisite portion, and lay the cut edge nicely down to the margin of the space needed to be open ; to continue to do this until the splint is completed, leaving the aperture with the lint lining at the bottom. This lint is now to be pinched up *in the centre* with a pair of dissecting forceps, and punctured like a hernial layer. The point of the scissors is then to be entered here, and the lint divided from the centre to the circumference, in a crucial form, reflecting the four angular flaps, which are then turned to the back, and fastened, with a little of the material used in forming the splint, to the margin of the aperture or foramen.

The opening made in this way is more evenly bevelled at its edges, and there is no secondary disturbance of the limb ; but Dr. Browne's plan has proved most successful in his own hands, and I have to thank Dr. Browne for the suggestion, which may prove useful to the readers of this paper.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 149.

36.—ON EXCISION OF THE KNEE-JOINT.

By Dr. John K. Barton, Surgeon to the Adelaide Hospital, and Lecturer on Surgery to the Ledwick School of Medicine.

[If the patient has reached forty years of age Dr. Barton considers that amputation should generally be preferred to excision, because a very great deal of reparative power is necessary to render a limb, after excision, a perfectly firm support. In a

markedly strumous constitution, although no symptoms of visceral disease can be detected, excision should never be performed, for inflammation in such constitutions, instead of ending with effusion of plastic lymph, usually furnishes a sero-purulent fluid. In cases where the patients have been hard drinkers inflammation generally runs an unhealthy course.]

Turning now to the local conditions which indicate or contra-indicate excision of the knee, it is evident that any disease of the joint which has produced suppuration, is just to the extent to which suppuration has gone, unfavourable for excision. Not only does abscess, in or about the joint, indicate a constitution unfavourable to excision, but it is generally the result of extensively diseased bone; and when we have evidence of the ends of the bones being extensively diseased, I believe amputation is the only operation advisable, for two reasons—first, excision would most likely fail to produce a sound or useful limb—and, secondly, if solid union was obtained between the bones, the amount of shortening would be so great that the limb would be of little use, its questionable advantages (being five or six inches shorter than the other) by no means compensating for the six months additional time spent in recovery. Necrosis of part of the head of the tibia or abscess in this bone, do not constitute states suitable for excision, as in order to get healthy bone for union, two to three inches of the tibia must be removed, which with the piece necessarily removed from the femur, would make the limb too short.

Of the three kinds of diseases of joints as described by Brodie—viz., that which originates in the bone, that which originates in the synovial membrane, and, finally, that beginning in the articular cartilages—the latter is the form of disease which offers the most suitable conditions for excision, for here the bone is but secondarily and to a small extent engaged, and there is very little tendency to suppuration. In acute inflammation of the joint, with suppuration, arising either from accident or otherwise, incision into the joint, to evacuate the purulent contents, will be the only operation required, and will probably be followed by anchylosis. In severe injuries to the knee, either compound fractures, implicating the joint, or severe lacerated wounds opening the joint, when it may be thought impossible to save the limb complete, it does not appear to me that excision will be at all applicable—for if the nature and extent of the injuries render the recovery of the limb hopeless, they would seem to me necessarily to forbid excision, the shock of which must be at least as great as amputation of the thigh. The experience of military surgeons who have attempted this operation for injuries in the field, is not encouraging. It should be borne in mind, when considering

the question of excision of the knee, that any case which does not result in giving the patient a perfectly *firm*, *straight*, and *healthy* limb, must be considered a failure. It is totally unlike amputation in this respect, which is always undertaken as a last resort, and is successful if the patient's life is preserved and the stump heals soundly. Viewing the matter in this light, it must be admitted, if the remarks I have made are well-founded, that the cases suitable for the operation of excision of the knee are rare. They will require—first, youth or a vigorous middle age; secondly, no constitutional disease, or signs of deteriorated blood; thirdly, the disease of the joint to be non-suppurating, and confined, or nearly so, to the articular surfaces. If from the cases which combine these conditions we subtract those of them in which a firm anchylosis can be obtained by rest and other suitable means, we have but few remaining in which the operation is called for; in these few, however, we have in excision the means by which we may not merely save the patient's life, but with his life give him back his limb; and it must be admitted that in doing this the operation well merits the encomiums it has received of being the most brilliant achievement of modern conservative surgery. The details of the operation itself, and the important after-treatment, seem now to be nearly perfected. All that we require to raise the per-centage of successful cases to three times its present ratio is a judicious selection of cases. —*Dublin Quarterly Journal*, *Feb.* 1865, *p.* 5.

37.—ON THE EXPOSURE OF ARTICULAR CARTILAGES IN WOUNDS.

By T. Holmes, Esq., M.A., Cantab., Surgeon to the Hospital for Sick Children, and Assistant-Surgeon to St. George's Hospital.

Some surgeons believe that to leave articular cartilage exposed in a wound is dangerous. This idea appears to be derived from the false analogy of wounds of the knee-joint, and of the symptoms attributed to inflammation of its cartilages. It is remarkable that the every-day experience of amputations performed through joints should not have shown that it is an error. A wound almost surrounded by exposed articular cartilage will heal without any bad symptom. Thus, in a case in which I excised the astragalus and scaphoid bones, leaving exposed in the wound the whole cartilaginous surface of the tibia and fibula above, the cartilaginous facets of the os calcis below, and those of the three cuneiform bones in front, all of these cartilages being natural, no unusual amount of inflammation took place, and the healing process, though it went on slowly, met with no

impediment. In another case I had an opportunity of examining the stump of an amputation through the knee-joint, in a child between two and three years of age. The child died from constitutional causes forty days after the amputation; but the stump had healed seventeen days after the operation, with remarkably little inflammation; and the patella was found as freely movable in the stump as in the natural joint, nor was any trace of inflammatory degeneration of the cartilages noticed. Again, although the cases on record are too few to form the basis of a decisive opinion, it may be reasonably alleged as probable that a wound in which no bony tissue is exposed is more likely to heal more kindly and soundly, and is less liable to pyæmic infection, than one where a large surface of cancellous bone is laid bare, with all its cells cut open. How often we see such wounds sluggish and refusing to unite. How often the divided bone participates in the inflammation going on around it, softens, and finally becomes carious; or dies, and has to exfoliate. And if this is the case with healthy bone, when exposed and divided in operation, it is still more often the case with bone already softened by low inflammation.—*Lancet, Jan.* 21, 1865, *p.* 50.

38.—ON PENETRATING WOUNDS OF THE KNEE-JOINT AND THEIR TREATMENT BY IRRIGATION.

By Dr. William Newman, St. Martin's, Stamford.

[Many years ago Dr. Newman drew attention to this subject, narrating five cases of penetrating wound of the knee-joint, followed in every instance by speedy recovery. The following cases have occurred since that time.]

Case 1.—In January 1862, I was asked to see a groom (a man over fifty years of age), who had, twelve days previously, fallen from a haystack upon the cutting-knife, and had laid open, by a slanting incision on the outside, his right knee-joint. The man had been kept quiet in bed, and the injury had been treated by poultices, leeches, &c.; but he was suffering considerable pain, had had very bad nights in spite of frequent opiates, while the joint was swollen, the integuments around puffy and infiltrated, and a gaping wound was freely discharging pus and synovia.

Irrigation was recommended, and continued steadily for twenty-four hours; so securing to the patient a good night's sleep, the first since the accident happened. After that time, from disturbing causes, other treatment was determined on, and employed. A week afterwards (nineteen days from the infliction of the injury), irrigation was again resorted to, at the patient's own request; and the progress to recovery was steady

and satisfactory. Sedatives were no longer necessary; the nights were good, and the wound healed. Constantly used at first, the water was, after some time, only allowed to drop on the knee when the joint became, even in a slight degree, hot and painful.

Case 2.—In May 1862, I saw a labourer, who had, two days before, sustained a punctured wound of the knee. The left knee-joint was distended by effusion, and painful in some measure; but the symptoms were not of the same urgency as those usually met with. Possibly, the extreme *nonchalance* and indifference which the man strongly showed existed physically as well as mentally; and his nervous system may have been singularly insusceptible of external impressions.

Irrigation was suggested, and carried out for six or seven days. At the end of that time, all joint-irritation had subsided; the swelling, also, had quite disappeared.

Case 3.—The details of this I propose to give at length, since some of the main points are well illustrated.

W. P., aged forty, agricultural labourer, came to my house on August 24th, 1864, having walked a mile from his own house to a side railway-station, and thence travelled by rail; the transit had given him a good deal of pain.

On the preceding day, August 23rd, he had sustained a punctured wound of the right knee-joint from a hay-fork. The point entered on the inner side, half an inch below the level of the mid-line of the patella, and passed upwards and outwards directly into the joint. He had walked a mile or more to his house after the accident. The joint was soon swollen, and became very painful, so as effectually to prevent his having any sleep through the night.

At midday on the 24th, when I saw him, the knee was much swollen and the joint tense with effusion. Pain was constant, and increased by pressure; and there was a blush of redness on the skin. He was told to return home, to go to bed, and to keep up constant irrigation of cold water.

August 25th. He had had some sleep in the night, and said he had much less pain. The knee was still hot and swollen. The treatment was continued.

August 26th. He was doing well. He had a threatening of shivering in the evening, which was stopped at once by taking some ammonia; no feverishness succeeded. The joint was easy, but still swollen.

The irrigation was continued constantly until the 28th.

August 28. The knee was less swollen, not painful, not hot. He slept fairly well. He was ordered to discontinue the dropping for an hour or two at a time; resuming it at once if any pain or heat should occur about the joint.

August 30th. The knee was much smaller; the patella a little thrown forward; otherwise, the external conditions differed very slightly from those of the uninjured joint. There was no pain on pressure; and very faint evidence of fluctuation. He was ordered to continue irrigation for three or four hours each day, with longer intervals between the applications.

Sept. 1st. He was going on well. The swelling was even less than when he was seen two days previously. There was no heat, nor pain. He was told that he might cease the water-dropping unless pain or heat should come on; and also that he might sit up a little, supporting the limb in its whole length. (For this he had special and clear directions.)

Sept. 4th. He had a relapse; was not nearly so well. The knee was more swollen, and was distinctly hot to the touch. This was easily explained. He had been up daily, and had not used any irrigation since the 2nd instant; but had been seated (contrary to orders), with the limb extended, and resting by the heel only, on a low stool, so having no support behind the seat of injury. Further, as the limb, from this new position, began to ache, and the knee became hot, he had been applying for the past twelve hours cloths wet with cold water, *propriâ manu*: and to do this had, on his own showing, moved his trunk forwards every five or ten minutes to reach the basin of water. I put a firm splint behind the knee, retaining it in place by a figure-of-8 bandage, sent the man to bed, and requested him to resume continuous irrigation.

Sept. 5th. He was better; was not in so much pain. The knee was still hot.

For the next seven or eight days, rest and continuous irrigation were insisted on; and, so much was the patient in fear that mischief in the joint might return, that he used the water even too thoroughly by voluntary increasing the number of points from which the water dropped. Hence, he induced a heat of reaction, transitory, however, and not accompanied either by pain or swelling.

Sept. 15th. He was ordered to discontinue the use of water entirely, and to move very cautiously, still wearing the back-splint.

Sept. 26th. The knee was normal in the size and appearance. He could bear a little weight on it, and was now beginning to walk a little.

Oct. 12th. He walked well; had nothing to complain of, and would go to work the next day.

I would remark on this case that the first error was one of distinct omission on my own part. I ought to have applied a splint behind the knee in the first instance, and so provided against the possibility of any ill result from thoughtless move-

ment. On this omission, in a measure, followed the relapse of Sept. 4th. More especially, however, was the recurrence of joint-irritation, due to want of care on the part of the patient. He failed to give to the limb, on first rising from bed, the support which he had been requested to insure; and hence the terminal points, hip and heel, alone having a resting place, the articular surfaces of the femur and tibia were in constant and close apposition at the knee, as the central point; these surfaces having been the seat of subacute inflammation but a few days previously. Again, too, the continual movement of the trunk necessitated by the choice of wet cloths in lieu of irrigation, destroyed the chance of rest to the injured part, and kept the articular surfaces of the bones constantly rubbing one upon the other.

No sooner, however, was complete rest insisted on, and cold thoroughly applied, than the threatening symptoms began to subside, and the progress to recovery was uninterrupted, though slow. There was still, on Oct. 12th, a shade of general thickening around and about the joint; but the movements were not impaired, nor was pain induced by extreme flexion or extension.

If cold be the appropriate remedy for any surgical affection, it is but a bare truism to maintain that it should be applied effectually. If this, then, is to be done in the case of some acute inflammation—of a joint, for example—the constant presence of an attendant will be imperative, who may apply thin coverings moistened with some frigorific or evaporating lotion to the part affected, and change these coverings at very frequent intervals, as they become either warm or dry. But to speak of such procedures as available or possible in everyday surgical practice would be absurd. How is cold then applied? Usually, a lotion is prescribed, and some material soaked in this is applied to the seat of injury. More frequently than not, and despite distinct orders, some three or four folds of material are used; thus more lotion is taken up, and the attendant's trouble is proportionably diminished. But the result is precisely opposite to that desired by the surgeon—the surface of the cloth very speedily steams with moist vapour, while the deeper layers are reservoirs of moist heat; practically, indeed, a poultice is concocted, without the efficiency of that time-honoured compound, and, this, too, employed under conditions where warmth and moisture are not desired.

This may be an extreme view; but I, nevertheless, claim for irrigation, or water-dropping, advantages which do not attend on the most careful employment of cold in the more ordinary forms. It is easily arranged; when once put in order, it is self-acting, even for hours; it is cleanly and effective. The absence or stupidity of an attendant will not interfere with the

application; and for its employment, no appliances are needed other than may be found even in the poorest cottage.

A reservoir of water (iced, if possible), a jug, or some other convenient vessel, may be hung to some hook in the ceiling; or tied to the frame of a bedstead.

The distance, from the injured part, of the water-supply, should not be less than three or four feet. If time be a great object in the treatment, or the symptoms of local injury be severe, a more speedy effect will result on the placing the jug at a greater distance above the bed on which the patient lies.

A few threads of lamp-cotton or Berlin wool, a narrow shred of flannel, or a strip of lint from the pocket-case, may do duty as a syphon. First well soaked in water, the shred must so be placed that one end lies at the bottom of the jar, while the other one hangs outside at a lower level. A continual dripping will so be kept up until the cistern is exhausted.

The injured part should be kept immoveable; the knee-joint should be fixed by a back splint sufficiently long and well padded, this being kept in place by a figure-of-8 bandage. The turns of the bandage must not be too tight, for, when wetted, the material shrinks very perceptibly; nor is it necessary to cover the joint itself with the folds. From hip to ankle the limb should be thoroughly supported; and, to insure this, the patient should be kept in bed. Waterproof sheeting, or an oil-cloth covering from a table, should be stretched beneath the limb, so as to prevent the wetting of the mattress; and, by adjusting this covering, a channel may readily be made, so as to direct the waste water into some dish placed on the floor.

Next, as to the time during which this mode of treatment may be carried on. I have continued the dropping uninterruptedly for eight or ten days more than once, and without ill result; then, intermitting the flow for an hour or two at a time, I have continued the same plan for a second similar period of time—three weeks in the whole. The time usually employed is about seven or eight days; and this may be extended or shortened according to the special requirements of each case.—*British Medical Journal, Dec.* 3, 1864, *p.* 623.

39.—ON THE TREATMENT OF CARIES OF SINGLE BONES OF THE TARSUS AND METATARSUS BY EXCISION OF THE ENTIRE BONE.

By T. Holmes, Esq., M.A., Cantab., Surgeon to the Hospital for Sick Children, and Assistant-Surgeon to St. George's Hospital.

Several objections occur to the proposal of removing tarsal bones entire instead of piecemeal. They are as follows:—

1. That the operation is a very much more severe one, and involves a more protracted convalescence. 2. That it is a very difficult surgical proceeding, and cannot be effected without injury to important vessels, nerves, and tendons. 3. That it leaves the foot in a less useful condition than the stump after amputation at the ankle. 4. That the same results can be procured by what is termed subperiosteal resection; that is to say, by scooping the whole bone away piecemeal.

I will examine these objections seriatim. First as to the severity of the operation. It cannot be denied that the total extirpation of any of the tarsal bones, and particularly of the os calcis or astragalus, leaves a very large gap to be filled up, and requires very extensive incisions; while, on the other hand, a carious cavity can be gouged out through a simple cut of moderate length. If, however, the latter operation is to turn out useless, its simplicity will be a poor argument for adopting it. These excisions in the tarsus do, indeed, involve a long convalescence: the foot cannot, in ordinary cases, be placed on the ground in less than a quarter of a year, even if everything goes on as favourably as possible; but they are not liable to be followed by much constitutional disturbance, and in healthy children recovery may be confidently expected. Besides, during the latter period of the treatment the patient need not be confined to bed, nor even prevented from walking about on a wooden leg. Therefore the difference of a few weeks in the time which the respective cavities take to granulate is of little or no moment, if it be true that in the partial operations sinuses are usually left communicating with diseased bone, and that the disease will spread again after the child has been running about for a little while, or after slight injury, while after the radical operation a sound cicatrix will be formed which will remain sound and healthly during the rest of life.

The second objection is to the difficulty of the operation and the importance of the parts necessarily injured. It is true that, in order to remove entire one of the larger bones of the tarsus, large flaps have to be made, and a somewhat formidable dissection encountered; but in children, at any rate, the operation need not last very long, and is not very bloody. I do not think that in any of the cases referred to below of excision of the os calcis or astragalus the child was under chloroform longer than a quarter of an hour; and in none of them did more than a single artery require to be tied. As to the parts which have to be divided, if the os calcis be removed by the operation described by Mr. Guthrie and devised by Mr. Hancock—i.e., by making a flap out of the heel like the posterior flap of Syme's amputation,—then the posterior tibial vessels and nerves and all the tendons which pass into the sole of the foot will neces-

sarily be cut through, and much loss of power in the foot will perhaps result, if not some loss of nutrition also. But none of these parts need be injured. The incisions by which the os calcis was removed in the three cases noticed below do not involve the injury of any considerable vessel or nerve whatever. The posterior tibial artery need not be wounded, and was not wounded in any of those cases; nor was it, in fact, usually necessary to use any ligature at all. Excepting the tendo Achillis, no tendon is cut but those of the peronei, and even these might possibly be spared. The operation is perfectly simple even when the calcaneo-astragaloid joints remain entire, and when these are destroyed by the disease it becomes of course proportionally easier. But it leaves a large bag in which pus collects after the operation, and usually requires other incisions for its release. Possibly, passing a drainage-tube through the cavity at the time of the operation would be a useful precaution; I have found much convenience from the use of such a tube in the after-treatment.

The operation for removing the astragalus when diseased is a more difficult one than that for excising the os calcis, and involves an incision across the dorsum of the foot, in which the anterior tibial vessels and nerve must necessarily be divided, and most of the extensor tendons. I have only once performed this operation (and then, I must allow, the operation was of doubtful necessity); but in that instance, although the thickening over the bones was very great, I found it possible to spare one at least of the tendons—viz., that of the peroneus tertius, and there was slight power of moving the foot after the child's recovery. In this operation also there is a very extensive gap to fill up, and a long period of treatment to be gone through; but the experience of those numerous cases in which this bone has been dislocated, and afterwards removed, shows that after recovery a very useful foot may be expected. It was the accidental occurrence of a case of this kind, under my own care at the time, which emboldened me to undertake the removal of the whole astragalus from its bed in this instance. I found the operation not very difficult by a curved incision, extending nearly from one malleolus to the other, and reaching forwards as far as the front of the scaphoid bone. The chief caution required in the operation is to divide all the ligaments which hold the bone in place, and dissect it clean on all other parts before meddling with its posterior surface, where the groove exists for the flexor longus pollicis tendon, near which the posterior tibial vessels and nerve lie. In this operation also I found no vessel, except the anterior tibial, which required a ligature. None of the other tarsal bones present any difficulty, unless it should be desired to excise the middle or external

cuneiform bone by themselves. But I am not aware that such cases ever occur. These bones, situated in the centre of the tarsus, are well protected from injury, and only suffer in affections of the bones around them.

The third objection which I have heard made to these operations is, that they leave the foot a less useful member than the stump of an amputation at the ankle. This is so perfectly the reverse of the fact that I should not have noticed the idea, had I not heard the objection from a surgeon for whom I have the highest respect, and did I not know that it influences the conduct of some justly celebrated operators—for instance, those of Mr. Syme's school. I have no hesitation whatever in saying that the foot, after the successful removal of the os calcis, or of the astragalus, and *a fortiori* of the smaller tarsal bones, differs far more for the better from the result of an amputation at the ankle, than it does for the worse from the natural healthy foot. In fact, in many cases of excision of the calcaneum there is very little difference from the healthy foot, except an elevation of the heel to the extent of about half an inch, and a consequent slight limp—not very perceptible if the heel of the boot be correspondingly thickened. The removal of the astragalus is a more dangerous proceeding, yet most surgeons have had an opportunity of seeing that after the removal of this bone, in dislocation, the foot remains perfectly useful. Another very important consideration, on which too little stress is ordinarily laid, is that no apparatus is required after these operations. In a discussion which recently took place at the Pathological Society, on the subject of amputation of the foot at the ankle-joint, I heard, with great surprise, one of the most distinguished practical surgeons present advocate the removal of one or two inches from the bones of the leg; thus converting the operation into an amputation of the lower part of the leg, with no distinguishing feature except that the stump (being furnished by the skin of the heel) might be more than ordinarily able to support the pressure of the wooden leg. But the main recommendation of Syme's amputation would be lost in such an operation. For in these amputations at the ankle, although some special form of boot is needed for continuous exercise, the great convenience is obtained that the patient can walk about the house with no apparatus at all—can start out of his bed and attend to any sudden emergency as well, almost, as if he were unmutilated. To a far greater extent is this the case after excision of the ankle, and still more after excision of the tarsal bones. Beyond a raised sole nothing is required; and even without this raised sole the foot is often quite useful enough to enable the patient to run nimbly. Therefore, I need hardly say that I consider this third objection to arise simply from want of experience of the operation.

That the same result can be obtained by subperiosteal resection, or by piecemeal scooping away of the bone, may or may not be true; but this consideration does not, I think, argue against this operation which it is the object of this paper to recommend. I do not desire to advise the removal of whole tarsal bones in any but those cases where a very large portion, almost the whole, of the bone is hopelessly disintegrated, and would seldom perform such an extirpation, except after a previous trial of partial operation (as gouging, &c.) Now in such cases, where all that remains of healthy or firm bone is only a thin shell of its outer part, I have no hesitation in saying that whether the same result could or could not be obtained by scooping the bone out, no better result could possibly be obtained from that measure than from total extirpation. Besides, the scooping process is at best uncertain; the extirpation, if the cartilaginous surfaces exposed be healthy, is almost sure to succeed. An impression prevails that the original bone is reproduced after the diseased bone has been scooped out and nothing left except the periosteum; but I cannot discover what support this opinion has in recorded cases. The possibility of scooping away the whole bone and leaving the periosteum has not yet been satisfactorily demonstrated; and if there is any case in which the bone has been proved by dissection to have been reproduced, I have failed to find it, though I have hunted diligently in English and foreign surgical literature for it. But after the extirpation of one of the tarsal bones, the resulting cicatrix is firm enough to support the weight of the body, and to give attachment and leverage to the muscles, and we should require very cogent demonstration to convince us that any bone which could be reproduced from the shreds of periosteum that would be left after the scooping process could replace the bone to any better purpose. The idea that after the scooping out of the diseased bone, a perfect case of periosteum, of the same shape and extent, is to be left behind, and that from this case the original bone is to be produced afresh, seems to me almost absurd. In the first place could the operation be so performed? Would it not rather result in leaving behind a shreddy mass, consisting only in part of periosteum, of which again a large portion would perish by the resulting inflammation? Nothing could be produced from such a mass except, perhaps, a few detached osseous nodules, and these would contribute nothing to the firmness of the cicatrix or the usefulness of the limb. On the other hand, is there not great risk, in these scooping or piecemeal operations, of leaving some of the bone which ought to have been removed? I pass over the greater loss of time and loss of blood which such operations entail, since this is really a subordinate part of the question. More blood is generally lost

in scraping away the inflamed bone than in exposing its surface by turning down flaps to remove it; but this would be of little consequence if the result were better. On the whole, however, I believe the attempt to scoop away the diseased bone, and leave behind the periosteum, will be found very liable to end in disappointment, and to allow of, or even cause, the extension of the disease to neighbouring parts.—*Lancet, Feb.* 18, 1865, *p.* 173.

40.—ON THE RESTORATION OF MOTION IN CASES OF PARTIAL ANCHYLOSIS, OR STIFF JOINT, BY FORCIBLE EXTENSION UNDER CHLOROFORM.

By William Adams, Esq,, Surgeon to the Royal Orthopœdic and Great Northern Hospitals.

[The interest attaching to this subject very much arises from its novelty, as it is only since the introduction of chloroform that we have been enabled to carry out this mode of procedure in an efficient manner.]

The operation of *forcible extension*, or *forced rupture*, as it is sometimes called, essentially consists in tearing through by forcible movements such bands of adhesion, whether internal or external to the joint, as may limit or prevent its motion; and in these operations we must necessarily tear through some of the proper ligamentous structures of the joint itself, especially the capsular ligament, which, being thickened and retracted by chronic inflammatory effusion, must be to a greater or less extent torn through before free motion of the joint can be obtained.

Bands of adhesion, thickened tissues, and ligaments are therefore necessarily torn through in these operations; and it would naturally be expected that such a proceeding would necessarily be followed by severe inflammation and serious risk to the limb or life of the patient; and undoubtedly such results have followed, perhaps even in many cases; and I shall allude to some such results in my own practice. But, on the other hand, I can adduce sufficient evidence to prove that when appropriate cases are selected, having reference especially to the constitutional condition of the patient and the cause of the anchylosis, and when the operation is done with proper precautions, and with less violence than is generally used, there appears to be no risk whatever of inflammatory mischief following the operation. Frequently, indeed, there is scarcely any heat or swelling, nor is the pain following it sufficient to interfere with the sleep of the patient.

The absence of inflammation, and more especially of suppurative inflammation, as a result of the operation, is in a great

measure to be explained by the subcutaneous nature of the injury; and in this respect it may be classed in the same category with dislocations and simple fractures, which we know are seldom, if ever, followed by suppurative inflammation; the great law of subcutaneous surgery being, that all wounds and injuries not exposed to the air, such as simple fractures, dislocations, &c., are not followed by suppurative inflammation; whilst open wounds—*i.e.*, wounds to which the air is admitted—such as compound fractures and wounds from accidents, are, as a rule, followed by suppurative inflammation. The truth of this great surgical law, first pointed out by Hunter, has long been recognized, and is known to be the basis of the practice of subcutaneous surgery, and the explanation of its success.

In the operation of forcible extension, the amount of injury inflicted upon the joint is undoubtedly much less than the joints sustain in many dislocations, especially of the hip and shoulder, in which not only are the capsular ligaments and other ligaments of the joints torn through, but the muscles surrounding the joints are often extensively lacerated.

The mode of procedure at the time of making the forcible extension is very simple and easily accomplished. The patient must be brought thoroughly under the influence of chloroform, and this must be carried to the extent of complete muscular relaxation. If the knee-joint is to be operated upon, an assistant must hold the thigh firmly, and the operator must first flex the joint, *i.e.*, bend it still further in its deformed position. It is seldom that much force is required to do this, and a sudden sharp jerk is much better than a steady gradual force.

When the joint has yielded to the flexion movement, the adhesions generally give way with a loud noise, which may even astonish the operator on the first occasion. The joint must then be forcibly extended or straightened, and this is often accomplished less perfectly, and with more difficulty, than the flexion movement. If the yielding of the joint is not satisfactory, a few more sudden jerks may be given, and the flexion and extension movement repeated; but at the first operation the surgeon had better avoid doing too much, as the amount of force used by some surgeons, in the expectation of obtaining the full amount of motion at the first operation, has undoubtedly been the cause of serious inflammation.

The joint should be firmly bandaged after the operation, and the limb fixed in a good position on a splint. The bandage may be damped by a spirit lotion. If swelling and heat follow on the next day, I resort to hot fomentations; but in a large number of cases even this is not required. The patient usually sleeps well without an opiate, and no swelling or heat occurs. If slight inflammation should follow, it generally disappears

about the third day, and then passive motion should be commenced. In private practice I generally obtain the assistance of a professed rubber, or shampooer, but a good deal may be done by the patient as well as the surgeon; and there are various mechanical contrivances, such as those suggested by M. Bonnet, by means of which the patient can exercise the joint.

In the case of a lady whose knee-joint I broke down recently, no swelling, heat, or pain followed. She walked into my house four days after the operation, without even a bandage on the joint, and I at once recommended her to be shampooed by Mr. Mahomed, of Somerset Street, Portman Square. Since then I have seen this lady, and the shampooing will be regularly continued. The operation has generally to be repeated several times, and this I do at intervals of about a fortnight.

One of the most successful cases I have had occurred in a man æt. twenty-seven, F. J., admitted into the Royal Orthopœdic Hospital, June 1, 1859. The patient had anchylosis of both knee-joints in the straight position after gonorrhœal rheumatism; the operation of forcibly flexing the joints was repeated fourteen times, generally one leg only being done at a time. This man left the hospital in June, 1860, now five years since the operation, can move both knee-joints freely, and kneel to work at his trade—that of a carpenter. He can flex one knee, so that the heel moves through a space of eighteen inches, and the other fifteen inches.

Now, with regard to the applicability of the operation to different classes of cases, I will offer a few observations; and for our present purpose will arange all cases of anchylosis, or stiff joint, in three classes, viz.—1st. The Strumous; 2nd. The Rheumatic; and, 3rd. The Traumatic.

With regard to the first class, the strumous, the result of my experience has been to prove that they are decidedly the most unfavourable for the treatment by forcible extension; and I have determined never to repeat the operation in any case belonging to this class, unless it should be of an exceptional character, and present some unusually favourable conditions, especially such as lead to the conclusion that the articular cartilages have not been the seat of ulceration.

In any scrofulous disease of a joint, where there is reason to believe that the articular cartilages and other textures of the joint have been much damaged—and be it remembered that in scrofulous diseases the articular cartilages are, as a general rule, destroyed to a greater or less extent—a stiff joint is certainly the best possible result for the patient; and to obtain this, therefore, should be the object of the surgeon, every care being taken to obtain anchylosis with the limb in the most useful position. Any attempt to recover the lost mobility can only

be made at the risk of producing serious inflammation; and if free motion should be obtained under chloroform, and no serious inflammation follow the procedure, the motion will not be permanently retained, owing to the damaged condition of the joint. Stiffening will gradually return, and the case must at least be considered a failure.

In one case belonging to this class in which I operated against my inclination, but at the request of the patient, after telling him the risk, suppuration of the knee-joint followed, and amputation was obliged to be resorted to.

In another case in which forcible extension was applied to the hip-joint, in a young lady, suppuration followed, but the patient recovered.

In some of these cases, occurring as they generally do in young subjects, there is a risk of separating the shaft of the bone from the epiphysis, which has repeatedly occurred, and sometimes even the bone may be fractured. It occurred to me on one occasion to break the thigh-bone in a young lady. The fracture occurred in the upper third, and as it was in a case of extreme contraction and deformity of the limb, I was thus enabled to produce a great improvement, by setting the fracture with the limb in a good position, so that the general result of the case was satisfactory, and I did not expect to obtain useful motion. The operation was undertaken to improve the form and position of the limb, and this was accomplished, though not in the way proposed.

I believe it to be in the strumous class of cases that the largest number of accidents and unfavourable results have occurred; and as a general rule of practice, therefore, I consider that the operation of forcible extension is not applicable to this class of cases.

With respect to the *second class of cases*—the Rheumatic—the result of my experience has been just as favourable as it has been unfavourable in the strumous class. In a large proportion of cases of stiff joint, or anchylosis, occurring in young adults after rheumatic fever, or as the result of that severe form which occurs during the progress of gonorrhœa, and hence we call gonorrhœal rheumatism, free and useful motion may be restored by forcible rupture of the adhesions and thickened ligamentous tissues.

There is one great peculiarity in the rheumatic form of inflammation, in whatever organ or structure it may occur—viz., a remarkable indisposition to suppuration; and this is in the highest degree favourable to the operation of forcible extension of joints. The tendency of rheumatic inflammation is to the adhesive form, so that whether it occurs in a joint, in the pericardium, pleura, or in the eye, where we can best watch

its progress and results, adhesions are formed, but suppuration does not occur.

I have never seen suppuration occur as the result of rheumatic inflammation in any organ or tissue of the body, and such an event must be extremely rare. Where it does occur, in all probability the case is one of a mixed character, and not purely rheumatic.

I have already alluded to the man in the Hospital with anchylosis of both knees in the straight position, and in whose case a complete cure resulted, after fourteen times breaking down adhesions, and freely moving the joints under chloroform. This case belonged to the most favourable class, and the treatment was commenced about three or four months after the stiffening had taken place, and steadily persevered in for twelve months.

At the Great Northern Hospital, I have recently had under my care a young woman, whose right hip-joint had been anchylosed in the straight position for a year, after an attack of rheumatic fever, in which both the knee-joints had also suffered. When fully under the influence of cloroform, the hip-joint was examined by several of my colleagues, and not the least movement in the joint existed. After several unsuccessful attempts to flex the thigh by a sudden jerk, more force than usual was applied, the pelvis being firmly fixed, and the anchylosis then yielded suddenly and completely, with a loud noise. Motion at the joint was at once free in every direction. No bandage or splint was applied, slight local inflammation followed, with a little increase of heat and swelling, all of which disappeared on the third or fourth day, cold lotion being applied. Pain, however, was considerable, especially when the slightest movement was attempted.

In a fortnight the operation was repeated, and the joint moved freely, without any force being applied. The sensation conveyed to the touch was that of one cartilage surface moving on the other, the surfaces, however, not being smooth. No inconvenience followed this operation, and the joint could afterwards be moved a little without chloroform, but any attempts at voluntary motion were always painful.

In another fortnight the operation was repeated, and the joint not only moved freely, but the cartilage surfaces appeared to be perfectly smooth and natural; nothing could be more satisfactory. No swelling or local symptoms followed. Passive motion was employed by a variety of means, especially by flexing the body upon the thigh, in the standing position, when the foot was raised and placed on a low chair, with the knee bent. She could do this very well, and Mr. Willett, the house-surgeon,

used frequently to flex the thigh when the woman was lying on the bed. But still all attempts at walking were painful.

Under chloroform, which was again employed, the motion was as free as in a healthy hip-joint and I never saw a case which promised a better result; but the woman could not be persuaded to persevere in the treatment, as she had a great dread of the chloroform, and each time suffered from its effects for several days. She was a highly hysterical subject, and the pain in the attempt to walk seemed partly due to this cause. The muscles, however, had not time to recover from the effects of a year's inaction. This patient, therefore, left the hospital with directions to persevere in the passive motion; and if shampooing, hot and cold douches, &c., could have been added, as in such a case they would have been in private practice, muscular power might have been restored more quickly. The head sister of the hospital, however, tells me that there was a considerable amount of deception practised by this patient, and that she had sometimes been seen to walk about very well, and on one occasion even was said to have danced. It is supposed that this woman wished to remain an object of sympathy and charitable support, which had been extended to her by a wealthy and benevolent lady. Of this it was difficult for me to judge, but the treatment appeared to be eminently successful, and there seemed no reason why the voluntary motion should not be more free.

In many of these cases movement of the joint may be restored by passive motion alone, without chloroform, if commenced at a sufficiently early period, *i.e.*, soon after the inflammation has subsided: and no fear need be entertained of again setting up inflammatory mischief, which deterred the older surgeons from adopting the practice, and consigned many patients to the consequences of a permanently anchylosed joint. Here the distinction is of the utmost importance between the rheumatic and strumous affections, as in the latter class the practice would often be as dangerous as it is safe in the former.

In July, 1858, a gentlemen came under my care, who had previously been attended for some months by the late Sir B. Brodie for acute rheumatic inflammation of the right knee-joint which had followed an attack of gonorrhœa. All inflammation had subsided when I saw him, but the knee had remained in a bent position and immovable. This gentlemen arranged with much ingenuity several pulleys fixed to the wall in his library; and by means of a number of ropes, straps, and pads, so placed as to fix the thigh and make flexion and extension of the knee-joint, set himself to the task of obtaining motion, whilst he was seated on the floor. With great perseverance he continued this plan for three or four months, sometimes repeating the move-

ments two or three times a day, and the result was completely successful. He recovered the full range of motion in his knee-joint, and is now able to ride or go through a hard day's shooting without inconvenience. In this case sulphur taken internally appeared to be useful in removing occasional rheumatic pains, and after steaming the joint, a turpentine or camphor liniment with friction was also employed.

It is not my intention to refer to individual cases, but if space permitted I could adduce a large number of instances in which the treatment has been to a greater or less extent successful in this, the second class of cases, in which anchylosis has been the result of acute rheumatism in young adults.

From my experience, it would seem that in this class of cases we may always depend upon finding the articular cartilages in a healthy, or nearly healthy, condition—a point both of pathological and practical importance. The anchylosis in such cases evidently depends upon inflammatory thickening and retraction of the ligaments of the joints and other fibrous structures surrounding the articulations. I doubt the frequent existence of intra-capsular adhesions, so often described by my colleague, Mr. Brodhurst, as existing in the rheumatic as well as in the scrofulous cases of anchylosis. In cases of fibrous anchylosis after scrofulous disease, by which the articular cartilages have been more or less destroyed, bands of fibrous tissue are always seen passing between and connecting opposed bone surfaces, and all such cases are essentially unfavourable for treatment; but adhesions passing between and connecting articular cartilages are rarely, if ever, met with independently of the ulcerative process; and I am not aware that the ulcerative process ever occurs as the result of acute rheumatic inflammation, either in the joints or other textures.—*Medical Mirror, April* 1865, *p.* 193.

ORGANS OF CIRCULATION.

41.—ON THE COMBINATION OF DISTAL WITH PROXIMAL COMPRESSION IN CERTAIN CASES OF ANEURISM.

By Dr. J. M. O'Ferrall, Chief Surgeon to St. Vincent's Hospital, Dublin. Reported by D. B. Hewitt, Esq.)

I have long since been of opinion that compression of an artery on the distal side of an aneurismal sac should precede or accompany that on the cardiac side of the aneurism. I have often observed arrest of pulsation easily accomplished, attended at the same time with a flaccid state of the sac; but I have also remarked in such cases that the moment the pressure ceased the sac filled and throbbed as before. The formation of the coagulum

is thus desirable as the first step to consolidation. A half empty sac is the ready recipient for the slightest thready current that can follow the cessation of the compressing force on the cardiac side. It, therefore, appeared to me advisable to interrupt the current only when the sac was full of blood.

These considerations, together with the knowledge of the fact that ligature of the artery on the distal side has sometimes cured an aneurism when the upper or cardiac portion of the vessel could not be reached, made me resolve to try this expedient whenever a fair amount of pressure was not followed by success.

From what I have observed, I am inclined to think that cessation of pulse in a sac which suddenly diminishes in size and becomes flaccid is less likely to be followed by a permanent cure, whatever time may have been occupied in the compression, and that a sac which retains its volume, and is, moreover, full of coagulum, is less likely to be refilled, however short the duration of the compressing force.

When the supply is completely cut off by ligature of the trunk, I believe that the danger of relapse from refilling of the sac by collateral sources is more likely to occur when the sac collapses and becomes flaccid at the moment of deligation than when its dimensions are unchanged. This firmness of the parietes of the sac is always considered favourable to the success of the operation, as implying the presence of fibrinous deposits, whether we attribute them to stasis of the blood or to inflammatory exudation, as suggested by the researches of that distinguished surgeon, the late Dr. Abraham Colles. I have no doubt, however, that whether a sac be filled by fibrinous deposits of some duration, or by coagulum recently formed, that a full sac is very influential in preventing the ingress of blood from a compressed artery above, or collateral branches from below; the compression below the sac need not, in some cases, last more than a few minutes before the current above is stopped. If, on making the pressure above and arresting the pulsation the sac remains full, the object is attained, and time is merely required to allow the blood then liquid to coagulate in the sac. In other cases, it may be prudent to continue both compressions for a longer time.

Three cases have already been treated on this plan with success. The first was published by me in the Dublin Quarterly Journal for November, 1846.—*Dublin Medical Press, March* 15, 1865.

ALIMENTARY CANAL.

42.—ON EXCISION OF THE TONGUE.

By James Syme, Esq., Surgeon in Ordinary to the Queen in Scotland, Professor of Clinical Surgery in the University of Edinburgh.

[Mr. Syme had on previous occasions removed the tongue by operation, both cases, however, proving fatal, he determined not again to repeat the operation. In the early part of November last, however, he was consulted by a gentleman aged fifty-two, on account of a painful affection of the tongue, which rendered deglutition impossible, and death seemed impending from starvation. He determined to remove the tongue as the patient urgently desired it.]

Being thus as it were compelled to make another trial of excision, I carefully considered all the circumstances concerned that might tend to interfere with its successful performance. Of these the one which most prominently presented itself was the prevention of voluntary deglutition that must result from depriving the os hyoides of the power by which it is drawn forwards. In the common cases of cut-throat, where a large transverse wound is made into the pharynx, although the suicide rarely accomplishes his object in the first instance, he still more rarely escapes the fatal effect of pulmonary inflammation induced by irritation propagated from the larynx; and I did not forget that both the patients on whom I had performed the operation in question died from purulent effusion into the lungs. Instead, therefore, of cutting through all the muscles of the os hyoides, as had been done in the former cases, I resolved to retain the mylo-hyodei and genio-hyodei entire, and divide merely the attachments of the genio-hyoglossi. I also thought it would be better to perform the operation without chloroform, since the patient, instead of lying horizontally, might thus be seated on a chair, so as to let the blood run out of his mouth and not pass backwards into the pharynx.

The operation was performed on the 29th, with the assistance of Mr. Annandale, Dr. Sewell, and Mr. Cheyne. to the first of whom I am especially indebted for his able co-operation. Having extracted one of the front incisors, I cut through the middle of the lip and continued the incision down to the os hyoides, then sawed through the jaw in the same line, and, insinuating my finger under the tongue as a guide to the knife, divided the mucous lining of the mouth, together with the attachment of the genio-hyoglossi. While the two halves of the bone were held apart I dissected backwards and cut through the hyoglossi

along with the mucous membrane covering them, so as to allow the tongue to be pulled forward and bring into view the situation of the lingual arteries, which were cut and tied, first on one side and then on the other. The process might now have been at once completed had I not feared that the epiglottis might be implicated in the disease, which extended beyond the reach of my finger, and thus suffer injury from the knife if used without a guide. I therefore cut away about two-thirds of the tongue, and then, and being able to reach the os hyoides with my finger, retained it there while the remaining attachments were divided by the knife in my other hand close to the bone. Some small arterial branches having been tied, the edges of the wound were brought together and retain by silver sutures, except at the lowest part, where the ligatures were allowed to maintain a drain for the discharge of fluids from the cavity.

Next day I visited the patient, and finding him in all respects comfortable, inquired if he could swallow. In reply he pointed to a drinking-cup containing milk, and intimated that he wished it to be filled; then placing the spout between his lips, while his head was bent backwards, he drank the whole without any cough or sputtering. Having seen this, I felt assured that the result would be satisfactory, and was not disappointed, as everything went on well afterwards. The only inconvenience experienced was from the edges of the jaw being occasionally displaced; but this was easily remedied by an ingenious contrivance of Mr. Wilson, the dentist, who, finding that a silver cap inclosing the teeth, was not sufficient for the purpose, fashioned a shield of gutta-percha, embracing the chin on each side, and secured to the metal plate by a wire.

Under an ample supply of nourishment by milk, soup, and soft solid food, there was a rapid return of strength, so that an improvement in this respect was almost daily observable, and before the end of three weeks the patient declared that he had never felt better in his life. He returned to Manchester on the 23rd of January.

Excision of the tongue has thus afforded complete relief in a case of the most formidable and distressing disease. How far the relief thus obtained may prove permanent, and how far it may admit of being extended to cases of similar kind, are questions that can be determined only by experience. But the frequency of malignant growth affecting the tongue in an otherwise sound state of the system urgently requires the truth to be ascertained in regard to the value of a remedial measure; and if the operation is now as I trust it has been, freed from the chief danger attending its performance, facts sufficient for the purpose will probably ere long be accumulated.—*Lancet. Feb.* 4. 1865, *p.* 115.

43.—ON A NEW MODE OF TREATING DISEASES OF THE CAVITY OF THE NOSE.

By Dr. J. L. W. Thudichum, Lettsomian Professor of Medicine of the Medical Society of London.

[The treatment of diseases of the nasal cavity has hitherto been attended with very great difficulties, partly owing to its being inaccessible to topical applications.]

All difficulties are removed at one stroke by the discovery of Professor Weber, of Halle; that when one side of the nasal cavity is entirely filled through one nostril with fluid by hydrostatic pressure, while the patient is breathing through the mouth, the soft palate completely closes the choanæ, and does not permit any fluid to pass into the pharynx (a physiological fact thus far already discovered by E. H. Weber, of Leipzig, before 1847, and published in Müller's Achiv, 1847, pp. 351–354); while the fluid easily passes into the other cavity, mostly round and over the posterior edge of the septum narium, in some persons also through the frontal sinuses, and escapes from the other open nostril, after having touched every part of the first half of the cavity of the nose, and a great part, certainly the lower and median canal, of the second half. By means of the application of this principle to the treatment of diseases of the nose, it is possible easily and frequently to wash the nasal cavity, to disinfect and deodorize it, to remove the sordes which accumulate so easily in it, and to apply to its surface a great number of beneficial medicinal substances, so as to prevent acute affections from extending, and to incline them towards a speedy recovery; to stop hemorrhages, allay irritations, and subdue in a remarkable manner chronic affections of the Schneiderian membrane, so as to re-establish a perfectly healthy surface and normal condition of the organ of smell.

The apparatus.—A rod of iron or brass, thirty inches in length, is fastened upright into a heavily loaded foot, so as to form a firm stand. On this rod slides a nut, which can be fixed at any height by means of a screw, and carries an arm and ring, in which is cemented a high cylindrical glass vessel of a capacity of from one to two pints. The glass vessel is open above, and its cavity contracts within the ring in which it is fastened, here directly to pass into a small bore muzzle, to which a suitably-sized flexible india rubber tube, thirty-six to forty inches in length, is fastened. To the other end of the india-rubber tube a stop-cock is fixed; upon this a little cup-shaped collar, and upon this the cylindrical perforated nozzle of gutta-percha or of prepared india-rubber. If now the glass vessel is filled with fluid, and the little stop-cock immediately underneath the nozzle is opened, the fluid will escape at the fine

openings of the nozzle; and if the nozzle accurately fits the nostril, and the fluid is allowed to flow, the fluid will enter and fill the cavity of the nose, as will be more specially described hereafter.

Great care must be taken to ensure an accurate fitting of the nozzle to the nostril of the person who is to be operated upon, as if fluid escapes by the side of the nozzle it makes the operation difficult and troublesome. It is therefore necessary to have several sizes of nozzles to be fixed upon the stop-cock at will—for adults, sizes of diameters corresponding to the sizes 5, 6, and 7 of the thick probangs of instrument-makers; for young persons and children, very fine and more conical nozzles of india-rubber or horn. These latter small nozzles have but one central aperture: but the large gutta-percha ones are provided with four openings on the convex part, for reasons which a little practice will show as cogent; for as the nozzle has to be held in an oblique position, one or two openings are pressed against the membranous septum of the nose and closed, while at least one will be open and sufficient to afford a good stream of fluid, two or three giving a considerable current.

In order to avoid all possible chances of infection and ensure cleanliness, I lay it down as a desideratum that every person using the apparatus should have his or her own nozzle, to be used exclusively by that person. In dispensaries and hospitals, where this cannot be so easily effected as in private practice, the utmost care should be exercised to clean the nozzles, and particularly the little openings, from any semi-solid matter, which easily becomes firmly adherent to them. As the current is always directed outwards through the openings, there is hardly any chance of the interior of the nozzle becoming unclean or infectious. Yet it will be well to give to each patient, particularly if he be the subject of specific disease, his own apparatus. Even the suspicion that a patient might by accident blow into the tube and endanger his successor will thus be avoided.

The apparatus described in the foregoing has been constructed for me by Messrs. Weiss, with their accustomed skill and accuracy.

Of the fluids to be employed for rinsing the nose.—Pure warm water, when introduced into the nose by means of the apparatus, causes in most persons a very disagreeable sensation, ending in lachrymation and sternutation (or tears and sneezing), with subsequent copious discharge of watery mucus from the nose. If the quantity of water run through the nose be large, the "cold" produced thereby, including the change in the sound of the voice, may last for some hours. To avoid this objectionable symptom, it is best to employ solutions of common salt, or other

salts, of sugar, or milk, for rinsing the nose. In the course of practice, cases will arise in which all these solutions offer advantages. For general use, a solution containing one ounce of common salt in a pint of water is satisfactory. Some persons will bear less salt; others will tolerate more. Of this solution —having a temperature rather lower than that of the blood— from one to four, or, if desired, any number of pints may be allowed to flow through the cavities of the nose. It does not easily produce sneezing, rarely lachrymation, and hardly ever any subsequent symptom of cold in the head.

The saline solutions which, next to common salt, offer the greatest advantages are those of the common phosphate of soda and phosphate of ammonia, and soda. They can be used by themselves, or mixed with the common salt. Their alkalinity has a beneficial effect upon the irritated Schneiderian membrane, and dissolves or loosens any deposits of mucus or pus, which so frequently dry and harden upon the surfaces of the nasal cavity. When these solutions are made with the common hot water of kitchen boilers, they are a little turbid from phosphate of lime. The presence of this slight precipitate is no objection to its use. On the contrary, it is a convenient means of distinguishing this from other uncoloured solutions which may be used at the same time.

Of the fluids to be employed for deodorizing the nasal cavity.— For this purpose I have employed dilute solutions of permanganate of potash. This agent had done me such excellent service in removing the fetor of the mouth in cases of typhus fever, that I was induced to apply it for the removal of the fetor of ozæna, and with the most striking and immediate success. A solution of from one grain to ten grains in a pint of water is a good proportion, according to the severity of the case. The solution tastes alkaline, and acts as a feeble escharotic upon healthy, and particularly upon vascular or erythematous, parts. When the margin of the nostrils is excoriated, the permanganate colours the excoriated part brownish; but the effect of this is rather beneficial than otherwise, as the excoriated and coloured part dries easily, and after the shedding of the faint brownish pellicle, appears healthy.

Mode of applying these and other fluids to the nose by means of the apparatus.—The fluid, of the proper composition and temperature, is filled into the glass vessel, which is lowered for that purpose. All air in the india-rubber tube is now replaced by fluid, the escape of the air upwards being facilitated by gentle manipulation. The glass vessel is raised and fixed in the position which will give the desired pressure. A little fluid is now allowed to escape from the nozzle, to make sure that all air is expelled. The patient (or healthy person, if it is only desired

to show the physiological experiment) is seated in front of a basin, with his head and face slightly bent over it, the apparatus standing by his side. He is told to breath through his mouth exclusively, and to abstain from swallowing. The nozzle previously selected as of proper size, and connected with the apparatus and filled as described, is now inserted into one of the nostrils, and held there by the patient's hand of the same side. The little stop-cock is now opened, and after a few seconds a continuous and rapid stream of fluid is seen to flow from the opposite nostril into the basin below. Persons who have control over themselves will always bear the experiment as here described; but young persons, nervous females, and children become confused, begin to cry, or to swallow and breathe through the nose. In such cases the level of the fluid in the glass should be very little above the level of the introitus into the external ear, so that the fluid runs very slowly, or only drops out of the free nostril. The hand of the operator should be upon the india-rubber tube, to close it by compression the moment he sees bubbles come through the nostril, or perceives that the patient swallows or becomes confused.

It is always well to let the fluid pass at first under slight pressure, in order to allow sordes within the nose to be loosened and crusts of dried matter to be softened. When this has been effected, it is useful suddenly to raise the glass vessel, and produce a rapid stream, which will then scour the impurities away. In some cases I have done this repeatedly with success. The loosening of crusts and lumps of inspissated mucus is always attended with some irritation, and also with retardation and diminution of the current of fluid. The sudden increase of the pressure is the surest means of causing the least inconvenience to the patient, and effecting in the quickest manner the purpose of the operator.

It is also well to reverse the current now and then, as sordes are much better detached in that manner. If only one nostril is diseased, or the principal seat of the disease, I allow the fluid to enter by the opposite side, and to leave by the affected nostril. I then change the current, and filling the affected nostril, allow the current to leave by the healthy one. Thus half a dozen or a dozen changes may be usefully instituted. This reversal has sometimes the effect of throwing large lumps of inspissated mucus and pus upon the upper side of the soft palate; and as they are too large to be carried round the septum narium into and through the nasal canal by which the fluid leaves, they are taken into the pharynx, and immediately ejected by the patient through the mouth. This presence of lumps upon the soft palate is, therefore, sometimes a cause of a sudden interruption of the operation. After the removal of these lumps, the opera-

tion may be continued as before. It is really surprising what an amount of sordes will sometimes be removed from the nose by this rinsing process. Anyone who has seen it once will easily conceive the manner in which, by means of these constant accumulations, nasal diseases become chronic, incurable, and lead to fearful suffering and death.

The shape of the basin in which the fluid which leaves the nose is received, is a matter of some importance. A common washhand-basin will suffice in many cases in which the patients are not disturbed by the splattering of the fluid as it falls into the accumulating fluid in the basin. The splattering can be mostly avoided by so placing the patient's head that the current from his nostril runs along the side of the basin. But there remains the objection that the sordes, mucus, and dirty fluid are constantly before the patient's eyes. All these evils can be avoided by using a washhand-basin provided with a funnel shaped tin cover, similar in construction to, but larger in size than, a common spittoon. It is still better to use a high cylindrical vessel of white china—say a large jug of a capacity of from one to two gallons—for the reception of the fluid from the nostril, as the funnel shaped top can be made more inclined, thus ensuring a quieter flow of the water. For the consulting room it is most convenient to have the washhand-basin sunk into the stand, with the tube leading from the lowest point into a hidden receptacle. If it is desired to collect any of the crusts and secretions for microscopical examination, the aperture in the basin may be plugged, so long as the rinsing process continues. But even this I find unnecessary when the bottom of the basin is not too rapidly inclined, but rather flat. The mucous and inspissated discharges then adhere to the basin, the current from the nostril being insufficient to carry them down into the tube, and can be inspected, or removed with a forceps into a little white china dish and further examined. The inspection of these discharges is of some importance in a diagnostic point of view; for the nature and amount of the epithelium which they contain show us the special part of the nasal cavity from which they come. The upper regions of the nasal cavity are covered by a cylindrical epithelium, which mostly shows two or three layers of cells upon each other. The lower regions of the nasal cavity are lined with a ciliated cylindrical epithelium, the action of which removes particles of dust and fluid towards the nares. When the discharges are purulent, they consist of pus-corpuscles only, with hardly any admixture of cylindrical epithelium. This characterizes chronic ulcerations. When chronic ulcerations becomes extended, the pus is mixed with cylindrical epithelium which is thus being shed. When the affection of the nose is quite recent, no pus, but only cylindric

epithelia are perceived in the secretion. In cases of old follicular ozæna, the crusts have on their soft surface a villous appearance, being condensed mucus, indicating in some slight manner the distribution, size, and shape of the mucous follicles of the Schneiderian membrane. In cases of ulceration with rapid loss of substance, as in specific disease, elements of the deeper tissues, particularly fibrous structures, can be perceived in the discharges. When the nasal cavity is the seat of villous or other descriptions of ulcerated cancer, the elements of these tumours are found in the blackish-brown, thick, and extremely fetid discharges.

Of the medicinal solutions which may be applied to the cavity of the nose.—Although the solutions before enumerated act in a measure as alteratives, resolvents, and escharotics, and, therefore, rarely constitute a sufficient medical application by themselves, yet they are more frequently used for preparing the nose for the application of more energetic and specifically acting solutions. To this latter class belong the solutions of alum, sulphate of zinc, and sulphate of copper—the best astringents; the solutions of nitrate of silver and bichloride of mercury —the most suitable alteratives; and the solutions of chloride of calcium, in which suboxide or oxide of mercury is suspended in a finely subdivided state, together with the bichloride solutions —the best specifics. Of stimulating solutions, a mixture of eau de Cologne with water or salt-water is sometimes useful.

The probable concentration of these solutions can be surmised from the circumstance that the sensibility of the healthy nasal cavity stands about midway between that of the eye and the mouth. When the nasal cavity is completely filled with fluid, the specific sense of smell cannot any longer be exercised; even the solution of eau de Cologne is not perceived to be such when it once fills the nose. The sense of smell being thus entirely obliterated by the fluid contained in the nose, the reflex effects which substances may exercise by means of this sense are entirely absent; and the only impingement which the fluids can produce is upon the filaments of sensitive nerves coming from the fifth pair. It is owing partly to this circumstance that comparatively strong medicinal solutions are borne by the nasal cavity without great secretion. Another circumstance favouring the application of stronger solutions is the ready manner in which the healthy surface of the nose defends itself against irritating, chemically-impinging substances by means of a copious flow of mucus. Excoriated or ulcerated parts lack this power of rapid secretion; and hence they are affected by medicinal solutions much more than the healthy parts of the surface of the nasal cavity. What is here stated is the general result of experience and experiment; but, at the

same time, I must insist that the application of medicinal solutions in each case should be begun with the greatest caution, as individuals differ greatly in point of irritability of the nasal cavity. In the beginning, therefore, very dilute solutions of medicinal substances should be used, and their strength be increased gradually, after their effect has been well exhausted, by the use of greater quantities applied by a quick flow, or the use of smaller quantities in a slow current distributed over a longer time of contact.

Solution of alum.—Half an ounce of roughly powdered crystallized alum is dissolved in a small quantity of hot water, and the solution made up to one quart by means of cold and tepid water in such a manner as to ensure that the temperature of the solution should be below, but near to blood-heat. In superficial ulceration or blenorrhagic conditions this solution is well borne. Ulcerated parts, which before its application were red, mostly appear as white patches after its application, thus showing that the effect of the alum on the ulcerated surface has been considerable. When I was desirous to manage with smaller quantities of solutions, I have sometimes mixed a little permanganate solution with that of alum.

Solution of sulphate of zinc.—From a scruple to a drachm of the sulphate of zinc, dissolved in a quart of warm water, together with half an ounce or an ounce of sulphate of soda or sulphate of magnesia, gives a suitable fluid.

Solution of sulphate of copper.—Of this sulphate also from a scruple to a drachm, mixed with half an ounce or an ounce of soda sulphate or magnesia sulphate, can be dissolved in a quart of warm water.

Solution of acetate of lead.—Of this crystallized acetate from a drachm to two drachms, together with half an ounce or an ounce of crystallized acetate of soda, may be dissoved in the quart of warm water.

Solution of nitrate of silver.—Of this salt not more than from half a grain to a grain should be dissolved in each ounce of water. A quart of water, therefore, in which previously from half an ounce to an ounce of nitrate of soda has been dissolved, may receive from sixteen to thirty-two grains of the nitrate. In particular cases the solution may be made stronger. The nitrate of potash is not so good as the nitrate of soda, because it has slightly irritating qualities. When it is necessary to use it in an emergency, when soda nitrate cannot be had, the solution should be more diluted.

Solution of bichloride of mercury.—The greatest caution is necessary in the use of this agent, as it has a tendency to produce excoriations on healthy surfaces. The first solution to be employed should be one containing five grains of corrosive sub-

limate in a quart of water, in which an ounce of common salt is also dissolved.

Solution of chloride of calcium with suspended oxide or suboxide of mercury.—These fluids are the common phagedænic waters, or black and yellow wash, to which common salt has been added. Two drachms of calomel, twelve fluid ounces of lime water, one ounce of common salt, and twenty ounces of warm water, yield the black solution. One scruple of corrosive sublimate, one ounce of common salt, twelve fluid ounces of lime-water, and twenty fluid ounces of common warm water, yield the yellow wash These mixtures must be well agitated in the glass vessel while being allowed to run through the nasal cavity.

Sedative solutions.—Of prussic acid forty minims to the quart of warm salt-water, of tincture of opium two drachms, may be taken. These drugs may be added to some of the above solutions of metallic salts. But if this is desired, it is better to substitute a solution of morphia for tincture of opium. The prussic acid is incompatible with the copper, silver, and precipitated mercury solutions; it goes conveniently with the alum and common salt solutions.

Styptic or hæmostatic solutions.—Amongst these, ice-cold salt-water, containing an ounce of salt to the pint of ice-water, takes the first place. When this, after having been continued for a considerable time, is insufficient to stop the hemorrhage, a fluid ounce of the tincture of the sesquichloride of iron may be added to each pint of ice-cold salt-water.

Stimulating solution.—One ounce of eau de Cologne upon ten ounces of salt-water is a useful stimulant. Strong spirit of wine may be taken in place of the eau de Cologne.

I have now fully, and for some readers perhaps somewhat too explicitly, described a number of medicinal solutions which may with advantage be applied to the treatment of nasal diseases by the method in question. I was desirous to impress upon the memory of the reader the fact that I recommend only such solutions as are brought up to a certain specific gravity by salts which do not decompose the medicinal agents. There may be cases in which it is desirable to swell the Schneiderian membrane by watery fluid and produce endosmosis, and others in which highly concentrated solutions may beneficially be used to effect exosmosis and shrivel Schneider's membrane. These adaptations, and the various accommodations of the fluids and their degrees of concentration, I must leave to the skill and ingenuity of those who make use of this method. They will also probably multiply the resources of the rhinotherapeutic pharmacy, and thereby add to the success and certainty of this interesting method of treatment.—*Lancet, Nov.* 26, *and Dec.* 3, 1864, *pp.* 600, 628.

44.—A NEW MODE OF TREATING DISEASES OF THE CAVITY OF THE NOSE.

By Dr. Thomas Skinner, Liverpool.

In March, 1859, a lady patient consulted me about her waitress, an orphan, in whom she took great interest, and whose only fault was an exceedingly fetid breath. On making an examination. I found that she was the subject of chronic ozæna. One morning, when syringing her left nostril, an enormous plug of congealed muco-pus came out of the right nostril, followed by a copious stream of water. The conclusion I then came to was inevitable, so I changed the nozzle of my instrument to the right side, and the stream came down the left nostril.

Since that time I have had many similar cases to treat, and I have always followed the same plan, and with marvellous success. I quite agree with Dr. Thudichum when he says—"It is really surprising what an amount of sordes will sometimes be removed from the nose by this rinsing process."

The means which I have always adopted have consisted of a pint jug of tepid water (my consulting-room water-stand with hot and cold water and waste-pipe) and one of Higginson's invaluable syringes. Occasionally I have had to add a deodorant, and I know none better than the following tincture, a teaspoonful of it being added to a pint of tepid water:—Tincture of camphor, tincture of myrrh, of each three drachms; soap liniment, two drachms; glacial acetic acid, twenty minims; oil of pitch, one drachm.

I have never had any difficulty with any of my patients when I have taken time and moderated the force of the injection to what they seemed able to bear, always commencing with great gentleness, and inserting the nozzle gradually whilst the injection is proceeding. I have never required in the worst of cases to medicate the injections further than stated. Constitutional treatment has accomplished all else.

In conclusion, allow me to direct the attention of Dr. Thudichum and your readers to the *surgical* aspect of this simple discovery.

In June, 1860, the child of one of my patients was brought to me, a little girl two years of age, who had pushed an Indian bead up the right nostril. The mother had made vain attempts to pull it down, and succeeded in pushing it entirely out of sight, accompanied with profuse bleeding and terrific squalling. While the mother held the child's face over my basin, I forcibly injected some tepid water by means of Higginson's syringe up the left nostril, when the bead with one single compression of the elastic cylinder made its appearance in the basin.

There is a Yankee method of treating such cases, which, though inelegant, is still very ingenious. It is as follows:—An emetic is first administered to the patient; a pocket-handkerchief is then tied tightly over his mouth, and held there until the usual medicinal effect takes place. It is not difficult to "calculate" the result.—*Lancet, Dec.* 17, 1864, *p.* 708.

45.—ON THE TREATMENT OF CONGENITAL CLEFT PALATE.

By Dr. Norman W. Kingsley, New York, U.S.A.

[The physical comfort of an adult patient with congenital fissure of the palate is usually in no way seriously impaired, for he obtains such control over the remaining organs of deglutition as never to experience any great discomfort from the defect. But it is quite another thing in regard to the mechanical function of making articulate sounds.]

So far as I have read, it seems to me too little credit has been given to the velum by physiologists as an organ of speech. I would, therefore, claim for it that it exercises a more important office in the modulation of sound than any other organ, except the tongue. By it, in conjunction with the pharynx, the voice is directed entirely through the oral cavity; by it, in conjunction with the tongue, the voice may be directed entirely through the nasal cavity, and by it the voice may be divided and directed through both channels. On it depend the tongue, teeth, lips, and other organs of the oral cavity for their current of voice, out of which to make articulate language.

It is essential to the purity of any spoken language with which I am acquainted, that, to produce certain sounds, the passage from the pharynx to the posterior nares should at times be entirely closed, while for other sounds it should be entirely open, permitting the whole voice to pass in that direction.

And in this connexion let me not be misunderstood as confounding the uvula with the velum. So far as my observations extend, the uvula exercises no control over the voice; and so far as any injury to articulation might follow, the uvula may be entirely removed, providing the velum and the pharynx have the power of closing the posterior nares. With congenital fissure of the palate it is, therefore, physically impossible for a person to speak the same language of those with perfect organs; and in this way only, in many cases, is the deformity betrayed to the society in which the person moves. It becomes us, therefore, not only as matter of professional pride, but on far higher and more noble grounds, those of sympathy with a class of unfortunates, in many cases as intelligent, as refined, and as

sensitive as their more fortunate neighbours, though deprived by nature of one of the most delightful acquirements of man,—it becomes us to use every means in our power which shall give hope of their restoration to society. It is not uncommonly the case that a practice is continued because it has grown venerable with age, and will not yield to a better method through jealousy or pride.

In the treatment of fissured palate, the operation of staphyloraphy has long held its rank as the treatment *par excellence* for this deformity, and although, during all the earlier years of its history, it was accompanied with varying success, it has nevertheless been put in practice so often, that in a surgical sense little else can be desired or hoped for. Thanks to such men as Warren, Fergusson, Pollock, and others, who by their consummate skill have carried this operation to such a state of perfection, that they have proved all that can ever be claimed for it. Whatever may be the future of staphyloraphy, their operations will form a brilliant page in the history of surgery. That this should seem at first the most desirable treatment for fissured palate all must admit. What more natural than the suggestion to pare the edges and unite them by suture, and thus form a perfect septum in place of an impaired one? Thanks to the gentlemen before alluded to, their skill has proved in this country what cannot be said of my own: they obtain a union more or less complete in nearly every case. I have dwelt at some length in the beginning of this paper on the only object of any treatment of congenital cleft palate being with a view of improving articulation. How will staphyloraphy stand the test? Has it, as a rule, enabled the patient to conceal the evidence of the deformity from society? Or in other words, has it proved in any number of cases that it can be relied upon to enable a patient to articulate so clearly and distinctly as not to continually betray the defect? That it does improve articulation in many cases somewhat, and perhaps all more or less, all candid minds must admit, from the abundant statements to that effect. But there is no serious difficulty experienced from a fissured palate in deglutition, and the only discomfort worthy of notice is the mortification arising from unintelligible or defective articulation, how far does limited improvement in speech answer the end desired and hoped for, and how far does it justify a tedious and painful operation, so long as the deformity is still patent to all? In this connexion let me quote from the highest living authority.

"The grand practical object of this operation is to improve the voice and articulation. Defective deglutition from this malformation is what attracts the mother's or nurse's attention in early life. The cries of infancy are in nowise peculiar in

one; but when definite articulation commences, or rather hould commence, the value of an entire palate is then appre- iated. The air and sound, in passing outward from the larynx, scape in part through the nostrils by the split in the palate. A nasal twang is the result, and articulation as in the normal tate is impossible. Immediately after the operation the modi- ication on the voice can be at once detected. ..... Improved rticulation, however, comes slowly. Years, many years, are equired for distinct articulation; and after the most successful peration for cleft palate, months and years are required to lter defective sounds. Voice and speech have to be modified new. With some the changes come slowly and sluggishly; ut with others they are so rapid and perfect that within a few ears the original defect cannot be detected except by a prac- ised ear."

Out of the large number of surgical successes in this country, s it a rule that the patient is recompensed for the suffering un- ergone by the result obtained? I must confess I have no data rom which to answer these inquiries; but I *can* speak know- igly of results in the American States, and while I have seen iany cases where there was a good union throughout the entire engtli of the former fissure, I have never seen one where the peech was not defective, nor where the friends considered that t was *very materially* improved. And the opinions now enter- ained in France may be said to perfectly coincide with those eld in my own country. In the 'Lancet' of Nov. 19th, 1864, find a review of an article furnished by Mr. Pollock on Sta- hyloraphy for the fourth volume of "Holmes's System of urgery," from which I make the following extract:—

"It is, we believe, a fact that the success which French sur- eons have met with in this operation is so indifferent that it is ractically abandoned in the French hospitals, notwithstanding ne early trials by which Roux connected his illustrious reputa- ion with staphyloraphy. The French surgeons now entertain ne idea that, on the whole, the defect is best left to mechani- ans.....But it is a very doubtful question whether surgery exalted by thus surrendering its triumphs, and calling in the id of a mechanist to supply defects which may be remedied by heir surgical obliteration. Surgeons undertaking staphyloraphy hould, however, always bear in mind one circumstance con- ected with the results of the operation. The most complete uccess in restoring the palate by union of the cleft by no means mplies restoration of the articulation to a natural or intelligible andard. Long after, or even for ever after the success of the peration, in a surgical sense, the patient may continue to speak a manner hardly less unintelligible and disagreeable than efore. The use of the new palate can only be acquired by care-

ful and intelligent practice in the mechanism of articulation. Much perseverance and careful tuition are constantly necessary to attain this end."

I can readily conceive, however, of rare presentations where, with abundance of material, a long uvula, and a good union, without much injury to the muscles, clear and distinct articulation ought to follow; but I think I am justified in saying that in a large majority of cases such a result is impossible. My reasons for such a sweeping assertion are as follows:—The newly-formed septum is too short at the boundary at the fauces, and generally somewhat more depressed than it would have been if it had united in its formation, thus contracting the passage between the tongue and the velum; the power of the levator muscle has been much weakened by the knife; the newly-formed septum is either taut or paralysed: and from this combination of causes the passage to the nares remains permanently open, thus permitting a large volume of voice to escape without the power of the tongue to exercise any control over or modify it. The ability to articulate any spoken language correctly must remain an impossibility so long as the velum and pharynx are unable to meet, and at will shut off that passsge. I apprehend that on this point alone rest all the failures of whatever treatment; for the difficulties to be overcome at any other point are comparatively slight.

If staphyloraphy, then, comes so far short of accomplishing, in a majority of cases, all that is desired, we are very naturally led to inquire, Is there any hope from any other source?

Obturators have for a long time been resorted to for this object; and when we consider that probably the earlier applications of this instrument were for perforations of the palate induced by accident or disease, and that their good offices were made apparent immediately, it is not to be wondered at that much was expected from their use in congenital cases, and especially so when the true principle upon which an instrument should be adapted was so little understood. It is hard to reconcile the statements which have passed into history of the perfect results obtained by their use in congenital fissure. An obturator properly adapted as a bridge to span a simple perforation of the hard palate, from whatever cause induced, will accomplish all that any treatment can, but there its mission virtually ends. To claim that, with a fissured velum, the cap can be bridged over, or the posterior nares plugged, and perfect articulation follow, is to claim an absurdity—a total impossibility. A great deal of ingenuity has been wasted in their construction, and the claims for them must have been made in advance, without waiting for the proof.

In the present state of science and art, our only remaining

hope of relief is the substitution of an elastic artificial appliance to fill up the gap. This simple idea is not a new one, neither is the application of it of recent date. Elastic artificial vela have been made, or attempted to be made, of some form or other, almost ever since the use of india-rubber for mechanical purposes, and, for ought I know, of other materials long before. One thing, however, is certain: the application of suitable elastic material, in skilful hands, for the treatment of these cases, is capable of producing far more satisfactory results than have heretofore been derived from any other source. The indications to be fulfilled by such an appliance are: an obturator, or covering for the cleft, which shall be perfectly adapted to the muscles against which it is to lie; shall be flexible—susceptible of all the motions of the velum or soft palate itself; shall be durable; easily detached and replaced by the patient; and in a word, shall be so under the control of the surrounding muscles when *in situ* that the patient shall have the power, which in a normal state he would possess, of directing the voice at will either through the mouth or the nasal cavity, or both, as desired.

Such an instrument as that described has been brought to such a state of perfection that I have no hesitation in saying that it can be adapted to any case of congenital fissure of the velum that is usually seen, whether complicated with a fissure of the maxillæ or not. It can be made so as to be retained *in situ*, without danger of misplacement; can be worn all the time from the first hour without discomfort; is capable of being raised and depressed and contracted upon itself by the muscles embracing it; and is so simple that a child cannot disarrange it to its detriment. Such an appliance renders it perfectly possible for the patient to learn to speak well.

The physical and mechanical difficulties to be overcome in the adaptation of this artificial velum are a serious obstacle to the operator, for on the nice adjustment of the instrument to all the parts surrounding the fissure depends entirely the comfort with which the patient wears it, and the consequent use he will make of it.

The movements produced by the superior constrictor muscle of the pharynx upon the remnant of the velum in the act of deglutition will be remembered. In the proper adaptation of an appliance arrangement should be made for this movement. If not provided for, the act of deglutition would compel it to fold upon itself, and thus interfere with that function; or, if too unyielding, it would irritate the surrounding tissues until it could not be borne. To accomplish that object, the instrument is made so that its parts shall slide over each other like scales when the muscles contract the fissure. It is also essential that

it should be accurately adapted to the superior or hidden part of the cavity, as well as to the inferior or more exposed. To secure this adjustment, impressions of the whole cavity are taken in plaster of Paris; these impressions reveal the conformation of all those parts hidden from the eye, including the floor of the nares, the inferior turbinated bones, the vomer, the chamber, and the walls of the pharynx as far down as the fauces. From these impressions plaster models are made in the usual manner, to which models the artificial velum is adapted.

The material of which the velum is made is elastic vulcanized rubber, prepared with special reference to this object, and possessing sufficient flexibility to be carried by the muscles in any direction they act upon it; also sufficient elasticity to regain, and firmness to keep, its original position when the muscles are relaxed. It is so delicate in its structure that I have never known a single instance of irritation or inflammation of the tissue in contact with it when properly adapted.

It is supported *in situ* by resting on the superior surface of the palatine or maxillary bone in the vicinity of the apex of the fissure. It is retained by a very simple attachment of gold connected with it near its apex, and reaching to one or two of the teeth, with sufficient hold around the tooth to prevent its slipping off. But even the presence of natural teeth is not essential to retain it firmly and properly in its position, as in one case which I had under treatment the patient had not a natural tooth in her mouth, and an entire upper and under set of artificial teeth was adapted, and to the upper set of teeth was attached the artificial palate, which was worn with as much satisfaction as any case that has come under my observation.

There are many points of physiological importance developed by this experience which would be most interesting to dwell upon, did not limited space prevent my presenting them in full. To some of them, however, I must briefly refer.

The intellectual capacity of the patient exercises a greater control over the rapidity and amount in improvement than the peculiar physical conformation of the defect. A musical ear, cultivated to a nice distinction of sounds, is of material benefit in making the most of this appliance. The age should also be taken into consideration, and as early an age as the patient would take an interest in developing its benefit would undoubtedly be preferable. The improper position in which some of the organs of speech are placed in the efforts of the patient to articulate distinctly becomes so habitual as to be almost impossible to overcome, and manifestly the earlier the age at which this is attempted before these habits become firmly fixed the better. I have, however, in one instance, adapted an instru-

ment for a patient over thirty years of age, and in another for one over forty years of age, both of whom derived very marked benefit from its use within a few months.

Again, the sensitiveness of the individual to the defect, the mortification experienced in the exposure by their speech of this deformity, will prove a powerful incentive to their practice and the consequent rapidity of their improvement.

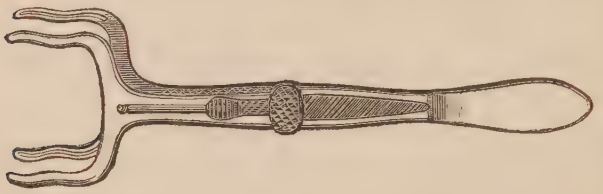
It is astonishing with what entire freedom from discomfort or annoyance the velum is worn immediately on its introduction. I have never had a patient where there was any irritation or inflammation in consequence of wearing it, and only in rare cases have they ever experienced a lameness of the surrounding muscles.

In conclusion, it will naturally be asked, What has been the result of this treatment in any number of cases? I can state most confidently, a decided improvement in speech within a very few weeks—a clearness and distinctness of utterance which the patient never showed before. In some cases this progress has been so rapid that within a period of six months after its first introduction the wearer would not, from his speech, be suspected of possessing such a deformity; and in *every case* within a few months the speech has improved so much as to render it perfectly intelligible to strangers, without repetition. It must not be supposed from these statements that there is any marked change in articulation immediately. There is almost always an immediate change in the tone of the voice, which is much less disagreeable than formerly, and this change is often mistaken for an improvement in articulation. These malformations are manifested much more in the attempt to articulate some languages than others. There is a most interesting field of inquiry for the physiologist growing out of this subject, which I have neither the time nor the ability to do justice to. I can only glance at it. In the articulation of the Anglo-Saxon tongue the compressor-nasi muscle is very seldom, if ever, called into requisition. In the cases of deformity which we have been considering, the escape of voice through the nostril is so great that the individual very soon forms the habit of using that muscle constantly. The result is, they acquire the power of making such sounds as do not belong to our language, but which nevertheless do form a very prominent part of other cultivated tongues. That muscle is much more frequently used in speaking the French language than our own. It will be readily understood, then, that these persons (other things being equal) can much sooner learn to speak the French language correctly than our own. With that language that habit assists them; with our own it must be broken up.—*Lancet, Dec.* 17, 1864, *p.* 686.

46.—AN INSTRUMENT FOR CONTROLLING HEMORRHAGE IN THE OPERATION FOR HARE-LIP.

By Thos. Smith, Esq., Assistant-Surgeon to St. Bartholomew's and the Children's Hospital.

The object of the instrument delineated in the adjoining woodcut is to facilitate the operation for hare-lip, by giving the operator a firm hold on the soft and yielding textures of the face, thereby rendering their division more easy; and, again, by controlling the hemorrhage, and thus enabling the surgeon to give his undivided and deliberate attention to the accurate and artistic adaptation of the denuded edges of the lip.



The instrument consists of a pair of forceps, of the shape indicated in the drawing; the blades are deeply grooved to hold the lip firmly, and one handle is fitted with a sliding catch and button to admit of the forceps being fixed, and the degree of compression regulated at will. It is used in the following manner:—The upper lip being separated from its adhesion to the gum to the required extent, one blade of the forceps is pushed up between the lip and gum, and the lip being grasped, the forceps are closed and fixed with the button; so that the fissure in the soft parts is surrounded, except at its upper angle, the coronary and other branches of the facial artery being thereby compressed. The operator, as is usual, should stand behind the patient, and having fixed the instrument, holding it by the handle, with one hand he may raise the lip from the jaw, while with the other he practises the necessary incisions. The edge of the fissure being pared, the sutures may be introduced, and the lip having been released from the gripe of the forceps, the sides of the cleft can be drawn together.

In some cases it is desirable to release the lip from the forceps before passing the sutures, in order that each suture may be passed through exactly opposite and corresponding points of the fissure with more certainty.

The above-described instrument, or one very similar, may be used for controlling hemorrhage in many operations on the lip or cheek, such as the removal of cancers or small tumours; it is

also available in operations on nævus of these parts, to arrest the circulation in the growth while the disease is either excised or ligatured. Especially I would venture to recommend its use when practicable for nævus of the face, treated by the injection of solution of perchloride of iron or other coagulating fluid, as I believe that thereby all risk of that rare though most fatal accident would be obviated—namely, the admixture of the injected fluid with the general stream of venous blood.

With the view of meeting the requirements of other operations on the face than hare-lip, Mr. Ferguson has been good enough to make an instrument similar to the one figured, but having movable arms, which expand so as to admit of adjustment to the extent of tissue requiring to be included within their grasp.—*Lancet, Nov.* 12, 1864, *p.* 545.

47.—ON CLEFT PALATE.

By Maurice H. Collis, Esq., Surgeon to the Meath Hospital.

[In *Retrospect*, vol. xxxiv., p. 180, will be found a previous paper on this subject by Mr. Collis.]

I will first give a short abstract of the progress of staphyloraphy and uranoplasty from the year 1819, when Roux operated on Stephenson, then a student in Paris, and afterwards Professor of Anatomy at Montreal. Prior to that date we have no records in connexion with actual operation, save that we know that in 1767 Lemonnier had operated for fissure of the soft palate with success, and that Græfe, of Berlin, had revived, or re-invented, the operation in 1816, but without a successful issue at that time. Roux's description of his first case reads like a romance, and is told with characteristic energy in his *Forty Years of Surgery*. Ignorant of Græfe's work in this direction, he was seized with the idea of the operation on the moment of first witnessing the deformity, and within two days he carried out his design. But ten days more elapsed when the patient, already cured, read a full account of his own case before the Academy of Science; and shortly afterwards, on his return to Canada, was appointed Public Lecturer on Anatomy—a satisfactory proof of the permanent benefit derived from the operation.

Roux's operations, with trifling exceptions, were confined to the soft palate; he was singularly methodical, and seems, in all cases, to have followed the same steps with unvarying regularity. First, he inserted, from behind forwards, the needles, armed with flat sutures of well waxed thread. Secondly, he pared the edges; and finally, he closed the fissure, if possible, by tying

the inferior, superior, and middle sutures in the order I have given. If the edges would not come together, from scantiness of soft parts, he made incisions, through the velum, along the posterior margin of the palate bone, leaving the aperture either to granulate or be closed by an obturator.

Dieffenbach, in 1834, improved on these incisions by making them through the soft palate, parallel to the edge of the fissure; and unquestionably this was a most important step in the right direction.

Up to 1842 all patients who had submitted to this wearying and weakening operation (as it then stood) were enjoined absolute quiet for many days, and were not allowed to swallow anything, not even their own saliva, until the edges of the fissure had united. Sir Philip Crampton was the first to break through this supposed necessity, and to feed his patients liberally with an abundance of soft and nutritious food; this was a most valuable improvement, as tending to bring about union by first intention.

In 1843 Mr. J. Mason Warren, of Boston, Mass., published in the 'New England Medical and Surgical Journal' the results of fourteen cases of soft and hard palate, treated by a new operation. Hitherto fissures in the hard palate had been closed by an obturator, or, in rare favourable instances, by the rude transfer of flaps from one or both sides of the fissure. I shall quote Mr. Warren's description of one of his operations, as I have not been able to get a copy of his earlier paper:—

"With a long spear-shaped knife, curved on its flat side, and cutting for half an inch on both edges, the mucous membrane was dissected from both sides of the palate, as far forward as the anterior angle of the fissure, and backwards to where the soft palate joins the ossa palati. A pair of sharp-pointed scissors, curved like a knife on the flat side, were now carried behind the soft palate, and the attachments of the latter to the ossa palati cut away, leaving the mucous membrane in front intact, thus affording a continuous flap. The uvula was now seized on either side, and the parts being made tense, the posterior pillar of the palate was freely divided, and the dissection continued backwards until the flap expanded, and not the least resistance was offered to any manipulations. Six sutures were rapidly introduced without difficulty."

In this extract Mr. Warren's two great points of improvement are shadowed out. First, the separation of the muco-fibrous periosteum of the hard palate from the bone, along the whole line of fissure, and outwards, on each side, as far as may be needed, even to the alveolar processes; and, secondly, the division of the muscular fibres of the palato-pharyngeus comprised in the posterior pillar of the palate. By this latter

process the soft palate is generally rendered capable of easy approximation, as, without doubt, these palato-pharyngeal fibres are the very strongest antagonists to this action. They draw the divided palate outward, and cause its edges to gape most widely, and are the special muscles for rendering the perfect velum tense. The levatores palati are less concerned in this divaricating action, inasmuch as their use is to raise the velum, and, in combination with the superior constrictor, to close off the passage into the posterior nares.

Warren's proposal to close the aperture in the bony fissure by a systematic dissection of the soft covering of the bones was a decided novelty, and a vast step in advance. It was based on the perception of the fact, that the imperfection in the vault of the mouth was due less to a loss of substance than to a misplacement; that, in fact, the coverings of the high but broken arch of bone might be brought down to a lower level, and there united without strain; that, to carry out the simile, a false ceiling might be put to the bony palate—a conclusion at which Mr. L'Estrange has independently arrived in this country. Warren's operations have been most successful; they form the groundwork from which all later improvements have sprung, if, perhaps, I except Mr. Fergusson's well-known and important one of dividing the levator palati muscles from behind. In one particular it is possible that we, who have since operated, have departed from Warren's proceeding without due consideration. It will be seen that he advises the completion of the operation at one sitting, and that to effect this he prolongs the dissection of the muco-periosteum backwards to the edge of the palate bones; and, then, with a curved scissors, divides the attachment of the soft parts to the posterior margin of these bones, leaving the mucous membrane in front intact. Now, without going into the question whether the hard and soft palate should always be attempted at the same sitting, it is manifest to all who have done the operation that Warren's method is the easiest to effect this object; the great point of difficulty is invariably at the spot where hard and soft palates meet. There is here, as a rule, such deficiency of the bone, from the angles being rounded off, as to make it almost impossible to complete the closing of the fissure when the operation is taken at two times. At least one may esteem himself fortunate, if there is not a pin-hole or something larger at this spot, which will only close after repeated touches of caustic or tincture of cantharides. Mr. Warren's method of dissection offers a better prospect of success, although I cannot say that the record of his cases is by any means as good on this point as I should have expected.

Mr. Fergusson, the author of the next improvement, founded it upon some dissections of a cleft palate, a rare advantage,

and one that justly gives his views the greatest weight, apart from his eminent position as a practical surgeon. He divides the levator palati, with a stroke of a rectangular knife, from behind forwards, cutting it above the level of the velum, where its belly is thickest, before it has spread out upon the velum. This incision, made with a knife whose point is out of sight, requires nerve and anatomical knowledge, and should not be attempted by any who do not feel themselves masters of the situation. To those who have studied the anatomy of the parts accurately, and who take the trouble of measuring, with their eye, the alterations which the bony points of the palate have undergone, or rather their deviations from a proper outline, there is no difficulty and no danger worth mentioning. I have frequently used Mr. Fergusson's knife and method for the right side of the palate, and Mr. Pollock's for the left, as much for the purpose of illustration as for convenience. Mr. Pollock, for the reasons I have given above, uses a knife either straight or slightly bent on the flat, cutting on both edges; this he plunges through the soft palate, close to the hamular process, and then raising the handle of the knife, without increasing his wound in front, he divides the spreading fibres of the levator on the back of the velum and any fibres of the tensor that may be in the way. His knife has the advantage that it suffices for all the steps of the operation in the soft palate.

I now come to Langenbeck's modification of Warren's operation. He uses very clumsy but useful round-edged chisels to separate the muco-periosteum from the bone. His plan of proceeding is good; his first incision is near the alveolar process, somewhat parallel to the fissure, but at as great a distance as practicable from it. This incision may be made with a common scalpel; his chisels are pushed into this incision down to the bone, and the muco-periosteum is forcibly separated towards the fissure. I have tried this method, and find it act more rapidly than Warren's plan of cutting outwards from the margin of the fissure towards the alveoli. Both may be combined with advantage in some cases. A further aid in shortening the operation (a very material point) may be had by using Mr. Philip Smyly's knife through the nostril. It is a small. square knife, set on the side of a long handle, and capable of being inserted by the nostril, and brought to work along the free margin of the bony fissure. It commences the dissection of the soft parts in this situation best of all knives I have used: and, in one instance, shortened the operation by at least ten minutes. Such are the principal means by which the difficulties of this operation have been gradually lessened, until it has become, in practised hands, one that presents no formidable difficulties, and that does not now necessitate any very lengthened endurance

upon the part of the patient. The following cases, exemplify the various steps of the operation in a practical manner, according as the malforation extends through the entire vault of the mouth, or is less extensive.

Case 3.—Dooley, a farmer, from Queen's County, aged twenty-seven, came to me, in 1856, with a huge fissure the entire length of the vault of the mouth. There had been hare-lip on one side only, which had been indifferently closed in childhood; or else from insufficient support, subsequently to the operation, the cicatrix had thinned considerably. The alveolar process had been originally widely cleft, and the edges, though now approximated, were not in contact.

March 5. I detached as much of the muco-periosteum as I could, working with curved knives, from the edges of the fissure outwards to the very margins of the alveoli. The flaps were scanty, and required much stretching to make them meet at all. Lateral incisions were cautiously made, so as to allow the flaps to meet easily for a few lines in the hope that a bar might be got across the gap, as a basis for further operations. However, the flaps were too feeble; there had been much hemorrhage, and much of them sloughed; so that when all had healed, in the course of two weeks, matters were much as before—no union taking place along the margins of the fissure. I consequently abandoned the hard palate, seeing that the gap was much wider than I could reasonably be expected to close. On the 19th I operated on the soft palate in the following manner:—I first passed a strong thread ligature through each half of the uvula, by means of a common Liston's needle; the ends of each ligature were brought outside the mouth, and given in charge of an assistant. Taking Mr. Fergusson's knife in my right hand I passed it into the mouth, and through the cleft, until it was beyond the hamular process. I now made the right side of the soft palate tense by drawing firmly on the ligature, which was previously passed through it, bringing the ligature well over to the opposite angle of the mouth. Turning the point of the knife upwards and outwards, I divided some of the fibres of the levator palati, and rendered the palate tolerably passive on that side. I next divided the posterior pillar of the fauces, containing the fibres of the palato-pharyngeus, with a snip of the curved scissors. This completely paralyzed the velum on this side. Before laying down the scissors I divided the palato-pharyngeus of the other side in the same manner. I now took up Mr. Pollock's knife. I made the left half of the palate tense, in the same manner as the right. Pushing the knife obliquely through the soft parts, just internal to the hamular process, until its point was seen in the fissure, I divided the fibres, which were expanded, fan-shape, on the

back of the velum, and was gratified to find this side fall quite passive. It is easy to test the efficiency of this part of the operation by simply crossing the hands, and seeing if the sides of the palate come well into contact without a strain. The sides of the fissure were now pared by a very sharp double-edged and spear-shaped knife. The advantages of such a knife is, that by thrusting it through the cleft edge of the uvula, then cutting up and down, the paring of the edge is rapidly effected. Three points of suture were inserted in Liston's needles, the material used being housewife thread. The greater part operated upon united, and the sutures were removed on the third, fourth, and fifth days—the lower first, the upper last. By a repetition of small operations, at short intervals, I was able to close the fissure up to a line on a level with the hamular process. Beyond this I could not advance, owing to the width of the gap and scantiness of material. However, the anterior margin of the united part became ultimately very firm, and almost straight, so that Mr. L'Estrange was able to adapt a gold plate, with perfect accuracy, to it, and so the entire gap was ultimately filled up. The satisfaction of the patient was extreme; the improvement in his voice was considerable even at once, and in about a year was still greater. Previously to operation he was scarcely intelligible, and in none of those operated on was the ultimate power of distinct articulation more increased. Considering the hopeless appearance of the gap at first, this result was as gratifying as it was unexpected.

Case 4.—Maryanne Elliott, age about thirty, cleft originally complete, but the gums had coalesced. The fissure ran along the centre of the soft palate and half of the hard, then turned to the left between the inter-maxillary bone and superior maxilla. The lip was not cleft. The two sides of the cleft had united in front. The fissure in the hard palate was high and narrow, and the soft parts were not too abundant, either where covering the bones or where free.

On the 21st March, 1862, I operated for closure of the hard palate (uranoplasty). I used Mr. Smyly's knife for the first time, and with great advantage. It is, as said before, a small square knife, set sideways on a long handle, capable of being introduced through the nostril. With it I commenced the separation of the moco-periosteum; it effected this rapidly up to a certain point, when Pollock's knife, used through the mouth, completed this part of the operation; the height and narrowness of the fissure made it hard to use any but small narrow knives, and the operation would have been immensely difficult but for Smyly's knife, which proved its value on this the first occasion upon which it was used. There was some strain on the flaps, rather from a tendency on their part to revert to their position

in contact with the bones, than from any deficiency of material. Lateral incisions, parallel to the fissure, failed to relieve this tension, and it was not until I had inserted, on either side, wedges of sponge, which not only pushed the centre of the flaps towards each other, but which also interposed a mechanical obstacle to the return of the flaps to their original site, that this strain was overcome. This idea of pushing the flaps, rather than pulling them, was the happy inspiration of the moment, and has since been adopted with great advantage. After forty-eight hours these sponges were removed. Union was satisfactory throughout all the hard fissure, except at its two extreme points. Anteriorly, in narrow fissures, it is hard to get raw surfaces in contact, in consequence of a redundance of tissue; and also because its density renders it unfavourable to union. At the posterior extremity the angles of the bones are rounded off; and there is almost invariably a deficiency in this point, such as often tries the patience of the operator. Mason Warren, Pollock, and myself have all had to complain of the difficulties of closing the lozenge-shaped gap or slit which lies at the border ground of hard and soft palate. Mr. Pollock has suggested a plan which seems efficient to close this gap. If the patient wear, for a time, a small plate, convex towards the mouth and concave towards the gap, there ensues a pouting of the lips of the fissure into the concavity, with a tendency of the sides to approximate and unite. In narrow slits I have attained the same end by touching the edges with strong blistering tincture. Where pin-holes remain in the pendulous velum this latter plan of closing is very good, and should be tried before the repetition of a cutting operation. To return, however, to the case of Maryanne Elliott, I was obliged to defer the operation for velosynthesis until the 23rd of April. It presented no special feature, and was performed as in the previous case, and with succes.

On the 14th May I closed a small lozenge-shaped gap between hard and soft palate, by a third operation. The pin-hole at the extreme front closed after a few touches of tincture of cantharides.

The speech in this woman had not been very bad, so that the immediate improvement, though considerable, was not so marked as in Dooley's case.

Case 5.—Miss S., aged fifteen, had a congenital cleft of the entire soft palate only, no hare-lip; the anterior end of the cleft was not acute, slightly rounded; a more easy looking case could scarcely be met with; yet we encountered unexpected difficulty from want of firmness, not so much in herself as in one of her friends, who insisted on being present during the operation. But for gentle perseverance and determination, and the kind

and patient assistance of Messrs. Smyly and W. Stokes, junr., I could not have attempted, much less completed, this operation. It was only necessary to pare the edges and insert the sutures, no division of muscles was required, and yet the operation was long and laborious. I passed a strong ligature through the thickest part of each half of the uvula, and by a steady hold of each, in turn, I was able to stretch it, so as to pare the edges. Five points of suture were inserted, some of them by means of an ingenious needle of Langenbeck's, which was of great service in this case, although in ordinary cases too complex and large. The upper, lowest, and middle sutures were of fine silver wire, and were inserted near the margin of the fissure, while the intervening sutures were of strong silk, and were made to include a good breadth of tissue, being inserted three or four lines from the edge; these last were removed in forty-eight hours, and the wires one after another; the last, near the tip, remaining in more than a week. By accident the tips of the uvula got slightly twisted, so that the left half came in front of the right, and as they were freely pared they united in this position, with a result which, though agreeable, was not anticipated. Usually, in the best cases, some notch remains at the tip of the uvula; here from the accidental superposition of one half in front of the other, the tip of the uvula, after all was healed, was delicately fine, and it would have required a close inspection to show that any operation had been performed. The conclusion of this case was as satisfactory as its commencement was unpromising.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 96.

48.—ON THE TREATMENT OF ENLARGED TONSILS.

By Dr. Philip Crampton Smyly, Surgeon to the Meath Hospital.

The subject of enlarged tonsils has met with much attention and extensive observations; and yet there are few diseases on which authorities differ so much as to the nature of the disease, the use of the organs in health, the effects produced by the disease on the system generally, or the connexion of these glands, whether diseased or not, with other glands, however remote. The indication for treatment is simple enough in theory, namely to diminish the size of the glands. Many and various are the means recommended; and the question as to whether the whole enlarged gland should be removed, or only a part, is not by any means settled. It is to a somewhat new mode of treatment I wish to direct attention in these notes.

When in London, last year, Dr. Mackenzie called my attention to a mode of treating enlarged tonsils by strong caustic,

following Fournié. Fournié employed Vienna paste and bichromate of potash. Dr. Mackenzie uses what he calls "London paste;" it consists of caustic soda and lime. I saw several cases treated by him, and have treated many in the same way myself. In most cases I believe it to be very superior to any cutting operation.

I generally employ a piece of ivory, slightly roughened and hollowed at the end. Dr. Mackenzie formerly used a simple glass rod, but now uses a very ingenious instrument, made by Krohne. It consists "of a small shallow spoon, or receiver, into which the escharotic is put; the receiver is provided with a lid or cover, which, when the instrument is grasped in the hand, can be pushed forwards or backwards by action of the index finger on a kind of trigger or half ring placed near the handle. The instrument is introduced closed, and, when opposite the tonsil, the lid is drawn back from the receiver, and the latter, with its contained paste, is pressed firmly against the hypertrophied gland for about five seconds; the lid is then pushed forwards over the receiver, and the instrument withdrawn."

This, of course, renders the application much more easy; though, with a little care, and a good light, such as is thrown into the pharynx by the laryngoscopic reflector, I find the piece of ivory quite manageable enough.

When applying the paste there are some cautions to be observed. 1st. The paste should be rather thick and tenacious, and not lumpy. 2nd. Only a little should be taken upon the holder. 3rd. The uvula should not be touched. 4th. Some vinegar should be at hand, in case any part but the tonsil be touched. 5th. The patient must not be allowed to use any water after the application, but allow the saliva to flow freely from the mouth; even after some time I have found a little cold water bring on very considerable pain.

I find from five to ten applications sufficient to remove the enlarged glands in most cases. The advantages appear to me to be:—1st. The application is less painful than cutting. 2nd. It is never followed by dangerous bleeding; and even a little blood is a great loss to many who have large tonsils. 3rd. There is little or no inflammation set up, and therefore less chance of the lung becoming affected, as pointed out by Stokes, 4th. There is no dread of a surgical operation. 5th. It can be applied to the youngest children, and 6th. The rapidity with which the parts may be removed. It would be tedious to give cases in detail; it is sufficient to say, that in all those I have treated in this way, I have not seen an unpleasant symptom.

Dr. Mackenzie, in comparing the "London paste" with

caustic potash, observes "that it possesses the following advantages:—

"I. It tends to penetrate, rather than to spread.

"II. The action, though less violent at the moment of application, continues for a much longer time.

"III. It causes much less pain."—*Dublin Quarterly Journal, Feb.* 1865, *p.* 45.

49.—ON THE TREATMENT OF PAINFUL CANCER OF THE RECTUM, WITHOUT OBSTRUCTION, BY ESTABLISHING AN ANUS IN THE LEFT LOIN.

By T. B. Curling, Esq., F.R.S., Surgeon to the London Hospital.

Amussat, in his well-known memoir, published in 1839, when describing the diseases which require the formation of an anus in the left loin, mentions *scirrhous affections of the rectum or of the large intestine which occasion great pain in defecation.* He remarks: "The establishment of an artificial anus is the only means of retarding the progress of the disease, and of prolonging life as much as if the cancerous affection occupied an organ less essential to the preservation of the individual." Though founded on a correct pathology, these views were never acted upon by their author, and made no due impression upon the profession. An exaggerated notion of the gravity of lumbar colotomy, and of the difficulty of opening the colon unless greatly distended, and an erroneous idea of the inconvenience consequent on an artificial anus in the loin, have hitherto limited the application of the operation to cases of long-standing insuperable obstruction, generally as a last resource. The distress, in cancer of the rectum, arising from frequent scalding discharges of blood and mucus with liquid fæces, from incontinency, and still more from the establishment of a communication, in males, between the rectum and urethra or bladder, with the escape of flatus from the urethra and of liquid fæces with the urine, and, in females, between the rectum and vagina, with feculent discharges from the latter passage, is of so urgent a character that it might be expected bold efforts would be made to prevent or palliate these evils. These were made, indeed, by Lisfranc and Dieffenbach in their attempts to excise the cancerous rectum, but they seldom removed the whole of the disease, and the patient, if he did not succumb under this dangerous operation, was left in nearly as miserable a state as before; so that the excision of cancer of the rectum has never obtained a place as an established operation in surgery.

The circumstance that lumbar colotomy is in no degree *curative*, but can succeed only in diminishing suffering, retarding the

progress of disease, and prolonging life, has no doubt weighed against the operation both with the surgeon and the patient. So long as it is viewed as one of great danger and of great difficulty, and so long as an anus in the loin is regarded as a disgusting infirmity, the objection will prevail. I have endeavoured, in a published work, to show the erroneous nature of these views. I have argued that the operation is not nearly so serious nor so difficult of performance as is commonly supposed, and that a well-formed artificial anus is attended with little inconvenience. If this be granted, the surgeon is scarcely justified in withholding from his patient even the temporary relief from the great miseries I have described which may be afforded by this operation.

In 1851 Mr. John Adams opened the sigmoid flexure of the colon in the left groin for the relief of obstruction consequent upon scirrhous disease in the rectum. The patient survived the the operation twelve months. In describing the case, Mr. Adams remarked on "the great relief afforded to the local disease by diverting the flow of fæcal matter into a new channel, thus removing one great source of severe irritation.

The first successful operation of lumbar colotomy to relieve the painful symptoms of cancer of the rectum *without obstruction* was performed, so far as I know, by Mr. Nathaniel Ward. I concurred with him in recommending the operation, and assisted in its performance. The patient was a man aged forty-five, in the London Hospital, who suffered considerable pain in the back and loins, particularly after an evacuation, which was attended with agonizing pain. In order to distend the colon, Mr. Ward passed the long tube through the diseased rectum and injected three pints of tepid water, and afterwards opened the colon without difficulty. The several symptoms were much relieved by the diversion of the fæcal matter through the artificial anus. The man survived eight months, and died exhausted.

The following case occurred to me at the London Hospital this year. The notes are furnished by Mr. Heckford, then house-surgeon.

Cancerous stricture of the rectum successfully relieved by making an artificial anus in the left loin.—W. B., a schoolmaster, married, aged twenty-nine, of dark complexion, and tolerably well nourished, was admitted into the London Hospital, under Mr. Curling's care, on the 12th of July, 1864, suffering from malignant disease of the rectum. There was no history of cancer in the members of his family, and his own health had been very good until sixteen months previously, from which time the symptoms of his present affection commenced. The first symptom noticed was constipation of the bowels. He was frequently obliged to have recourse to purgative medicines;

occasionally the bowels were unrelieved for six or seven days, and the fæces were always hard and lumpy. For the first seven months he managed to treat himself; but the costiveness becoming more obstinate, and a constant dragging pain and sense of weight being felt about the sacrum, and also round the anus, with an occasional discharge of blood, he applied to a surgeon. This gentleman did not examine the rectum; but said that it was probably a case of internal hemorrhoids. Soon after this he had an attack of erysipelas of the face, and fell into a low condition of health. He seems, however, to have entirely recovered from this, and at the time of his admission his aspect was certainly not that of one suffering from an exhausting malady. About Christmas, 1863, he noticed that the discharges of blood were much more frequent. Seven months ago he lost all power of retaining the fæces; and since then there has been a constant flow of feculent matter, mucus, and sometimes blood. His rest and digestion were also greatly disturbed.

He now came under the care of Dr. Guy, of Doncaster, who recognised the true nature of the case. For a time he used palliative measures, such as sedative enemata, &c.; but as the patient's health was giving way, Dr. Guy recommended him to come to London and consult Mr. Curling, which he did on the 9th of July last. Mr. Curling, being of opinion that he would derive benefit from an operation, advised his coming into the London Hospital. When he was admitted all the previously named symptoms were more aggravated, and, of course, he was unfitted for his usual occupation.

Mr. Curling detected extensive cancerous disease, reaching nearly to the anus and narrowing the passage. He was unable to pass his forefinger fully through the mass. During the first week he was ordered two doses of castor oil, which had the effect of bringing away a considerable quantity of retained feculent matter. He also had half a grain of morphia every night.

On July 19th Mr. Curling, with a little difficulty, passed a No. 1 bougie through the diseased mass. On the following day the operation was performed, chloroform being administered. The patient having been placed on his right side, a small tube was introduced beyond the stricture, and about three pints of linseed tea were injected into the bowel. To prevent its return the tube was kept in during the operation and its aperture plugged. A transverse incision, four inches in length, was now made midway between the crest of the ilium and the last rib. The different fascial and muscular layers were divided on a director until the subperitoneal fat was arrived at. As this was rather abundant, some little difficulty was experienced before reaching the gut, which was at the bottom of a deep wound.

The colon was found uncovered by peritoneum, and, having been drawn to the surface, a longitudinal opening was made in it, when a quantity of linseed tea immediately escaped. The gut and the lips of the wound were united by four sutures. For the first two days there was an almost uninterrupted flow of the intestinal contents, but at the end of this time an opposite condition supervened, necessitating a slight purge. For a time a charcoal poultice was applied. From the date of operation not a single bad symptom occurred. His appetite and digestion were good; he slept well, and was cheerful in temper. The wound granulated healthily, the sutures being removed on the fifth day. His former distressing pain in the back also entirely left him, and he was able to sit up in about ten days.

After the first three weeks a slight difficulty arose in the rapid contraction of the artificial anus. To remedy this dilatation was assiduously employed. Weiss's three-bladed dilator was introduced twice a day, and the aperture stretched; and at intervening periods pieces of laminaria bougie, properly curved, were passed into the bowel and retained there a sufficient time to distend the opening.

These means having proved effectual, he left the hospital on Sept. 3rd, and returned to his home in Yorkshire. His present condition, as described by himself, is most satisfactory. He writes, on Oct. 12th, thus:—"Since leaving the hospital I have been gradually gaining strength, and have not once felt the pain in my back, formerly so acute. There is a slight discharge of mucus from the natural anus, but no feculent matter. I have pains still at intervals in the rectum, but they are generally of short duration. The artificial anus has shown a tendency to contract, but to counteract this I insert my fingers occasionally, or use boxwood plugs night and morning. Its external edge is becoming very firm and strong, thus rendering it more difficult to stretch. There is a slight involuntary discharge during the day, making it necessary for me to wear a small bag lined with india-rubber. With this exception my motions are regular, twice daily, and I only take opening medicine at intervals when I find it necessary. I rest well and have a good appetite, and am both in appearance and reality healthier than before the operation. If altered at all in bulk, I am rather more fleshy, though I exhibit no signs of requiring the 'Banting treatment.'"

Dr. Guy, in a late report of his patient, dated Dec. 11th, five months after the operation, states that his appearance is healthy, that he has gained a stone in weight, and suffers very little pain, but experiences a sensation of heat and stinging around the anus, from which there is a little discharge of mucus.

This case was a remarkably favourable one for operation.

The patient was much younger than most persons affected with cancer of the rectum, and he had not suffered materially in his general health. It has improved since the operation, so that we may fairly anticipate that in addition to relief from, and prevention of suffering, the progress of the disease will be retarded, and that his life would be a good deal prolonged by the establishment of the artificial anus. The great diminution in the pain in the back in this case and in others after the operation shows that this symptom is greatly dependent on the passage of feculent discharges over an ulcerated cancerous surface.

I would urge the importance of not delaying lumbar colotomy till the worst evils of cancer are experienced, and till the constitutional powers have become seriously weakened by an exhausting disease. I have met with cases of genuine cancer of the rectum attended with very little impediment to defecation, and with very moderate local suffering. In such cases there is not sufficient call for the surgeon to operate; but when the local changes and symptoms clearly predicate the greater evils which will follow, it is far better to prevent their occurrence, by diverting the passage of the fæces at an early period of the disease, than to delay the operation till life is rendered miserable, and till the strength is so reduced that the operation must be performed under conditions less favourable to success. But even in an advanced stage of cancer of the rectum, when life cannot be much prolonged, the relief afforded by the operation is so great, and so much comparative comfort results, that patients in this state may be well advised to submit to it. The following case will bear me out in this remark:—

On Feb. 14th, 1863, Dr. West requested me to accompany him a short distance into Essex to see a lady who was labouring under obstruction of the bowels from a large scirrhous tumour in the anterior wall of the rectum, which compressed the bowel against the sacrum. A recto-vaginal fistula had formed, and before the obstruction, with the exception of very small quantities of liquid matter, all the fæces passed by the vagina. We found her in bed, weak and depressed, and suffering from constant sickness. There had been no action of the bowels for five days. I made a careful examination of the rectum, but could find no passage, the bowel being obstructed by a solid hard tumour of considerable size, which also closed the vagina. The sickness and depression rendered the case urgent, and an immediate operation was decided on. Chloroform having been administered by Mr. Davey, of Romford, the colon was opened in the left loin. The bowel was reached without difficulty; but it was contracted and deeply seated, so that it had to be dragged to the surface

before being opened. The margins were then secured to the skin with sutures. The irritability of the stomach quite ceased by the following day, and in two days she was able to take food freely. There was a copious fæcal discharge from the loin. The wound went on well, and healed in about a month, leaving open a sufficient anus. Towards the end of March a fistulous communication took place between the bowel and bladder, and urine escaped freely by the artificial anus, only a small quantity of flatus and urine, tinged with fæces, passing through the urethra. She became gradually weaker, and died of the cancerous disease on May 9th, having survived the operation three months. Mr. Davey wrote to me that she was almost entirely free from pain after the operation, and improved very much in spirits, complexion, and general condition; and about six weeks before her death she could walk about the house, and went out daily in a Bath chair.

I will conclude this communication with a few additional remarks on the causes which might deter surgeons from having recourse to lumbar colotomy in cases of painful cancer of the rectum—viz., 1st, the difficulty of opening an undistended colon without wounding the peritoneum; 2nd, the dangers of the operation; and 3rd, the inconvenience of the artificial anus.

A sound knowledge of the anatomy of the parts, some practice on the dead subject, and confidence in using the knife, are certainly very necessary to the satisfactory performance of this operation; but a skilful surgeon will seldom experience any real difficulty in opening a moderately distended colon. In Mr. Ward's case and in the case of W. B., related in this paper, some distension of the bowel was secured by previous injection through the rectum; and although this cannot always be done, as in the case I saw with Dr. West, the surgeon is not likely to be baffled in reaching and opening the colon outside the peritoneum. In three of the six cases in which I have performed lumbar colotomy on the adult, the colon was contracted, and in one case it was actually compressed against the spine, and put out of the way by the distended small intestines. There might be great difficulty in reaching the colon in a very fat person; but the subjects of cancer of the rectum are not likely to be in this condition. They are always emaciated more or less. A recent writer, Mr. Erichsen, in reference to lumbar colotomy in cancer, remarks—"In such cases the operation is very difficult in execution; for on account of the contracted state of the colon, that intestine recedes behind the quadratus lumborum, and the folds of peritoneum nearly surround or invest it; and it is so covered in by fat and cellular tissue that a very cautious dissection is required to expose it without wounding the peritoneum." Mr. Erichsen has clearly overlooked the circumstance that the

bowel can generally be distended artificially in these cases before the operation is performed.

The parts divided in the operation are skin, muscle, and fascia, and there are no vessels of any great size in the way. The chief risk is of wounding the peritoneum. In the nine operations which I have performed or assisted in, this occurred only once. It could not be avoided in consequence of the very contracted state and displacement of the colon, although the case was one of obstruction. But no peritonitis ensued, and the case did quite well. The success which has attended lumbar colotomy in persons weakened by organic disease and want of nourishment shows that it is not so formidable and dangerous as is commonly supposed. Indeed in no case which I have witnessed has death resulted from the operation.

An artificial anus in the loin, well established, is attended with little inconvenience or trouble in a healthy state of the alimentary canal. Fæces pass only at regular intervals, and the escape of flatus and feculent matter at other times may be prevented by a well-adjusted pad and bandage. When diarrhœa occurs, or unhealthy gases are generated, then annoyance is experienced. An artificial anus, however, under any circumstances, is much less inconvenient and annoying than a cancerous rectum with incontinency, or with such frequent and pressing desires for relief that the patient is constantly disturbed during the day, and is obliged to rise eight or ten times in the night. I was much struck with the little inconvenience attending an artificial anus in the loin in the case of a gentleman with impassable stricture of the rectum, and a communication between the bowel and the bladder, who was successfully operated on by Mr. Pennell in Rio Janeiro in 1849. This gentleman, a merchant, was afterwards much engaged in business, and went a good deal into society. In 1854 he came over to this country, when, through the courtesy of Mr. Clendon, of Albemarle-street, I had an opportunity of seeing and examining him. He died only last year of another disease, having survived the operation fourteen years. In the case of W. B., Dr. Guy, in a letter dated December 11th, states that the artificial anus looks well, and really seems to be assuming a certain degree of retentiveness—in other words, to allow the patient periodically to control the emptying of the bowels. The opening admits the finger, and is not at all sore or irritable. I have myself noticed in more than one case that when the finger has been introduced into the lumbar anus after the wound has quite healed, it has been grasped by the surrounding fibres of the abdominal muscles, much in the same way as by the anal sphincter when the finger is passed into the rectum.—*Lancet, Jan.* 7, 1865, *p.* 3.

ORGANS OF URINE AND GENERATION.

50.—ON LITHOTOMY AND LITHOTRITY.

By Holmes Coote, Esq., Surgeon to St. Bartholomew's Hospital.

The anatomy of the perineum has been made unnecessarily complicated by the arbitrary subdivision of the fasciæ into numerous layers, which do not exist in nature. We meet successively with the skin; subcutaneous tissue and superficial fascia; the muscular fascia; the layer of muscles—namely, erector penis, accelerator urinæ, &c.; the interosseous membrane or ligamentum triangulare. Beyond this lies the pelvic fascia. Remember these two points: first, the triangular ligament does not extend below the urethra; secondly, the vessels of the perineum are very irregular in their course and distribution. The distance between the neck of the bladder and the surface of the perineum depends upon the thickness of the subcutaneous strata, varying from two to four inches. The neck of the bladder lies about an inch behind the symphysis pubis, in which spot it is held firmly by its ligaments. It is a fault with many surgeons to cut too much backwards when the knife leaves the groove of the staff and runs between the bladder and rectum. The operation is always an anxious one, because the greater part is performed on structures which are out of sight; but, when difficulties arise, they almost invariably proceed from the fact that the surgeon has missed making a proper opening for the extraction of the stone. In this hospital, where high antiquity gives a traditional interest to its rules, it has never been in my remembrance the custom to draw off the urine just before the operation, and to distend the bladder by the injection of warm water. Such a proceeding is unnecessary and tedious, and is apt to disturb parts. But the rectum is always carefully emptied by means of an injection, and the patient is enjoined to hold his water as much as possible. The position of the patient is that commonly adopted elsewhere.

I do not recommend that the first incision be very deep. When the skin has been divided to the proper extent, the fat and subcutaneous tissue will readily yield to any amount of pressure. By adhering to this rule you often avoid wounding arteries of considerable size, or even a part of the bulb. The point of the forefinger of the left hand should then feel for the staff as it lies under the pubes, and the knife should be made to enter the groove by being pushed obliquely upwards, inwards, and backwards, so as to pass if possible *behind* the bulb and its arteries, being introduced about the middle of the external wound.

The membraneous part of the urethra is that usually first opened, and some urine often escapes; but do not hurry the withdrawal of the staff. Press the knife onwards, followed by the left hand, until you can get the forefinger well into the prostate, when in most instances you can dilate to a sufficient extent. I believe the more we dilate and the less we cut the better. In a case which I had the opportunity of examining, the membranous part of the urethra was opened, the prostate partially divided, and the neck of the bladder had the smallest possible incision. In the choice of instruments, every surgeon has his own fancy. I commonly complete the operation with an ordinary knife; but in cases of very deep perineum a beaked knife is to be preferred, and in this case it is perhaps better that, as he presses the instrument through the prostate, the surgeon should depress the handle of the staff with his left hand. The stone is removed either by the forceps or the scoop. It may sometimes be turned out by the finger. There is no need of alarm should a moderate amount of hemorrhage ensue; indeed patients in whom this occurs seem often to make the most rapid recovery. Death from hemorrhage is extremely rare. Do not introduce a gutta-percha tube into the bladder through the wound. If the incisions are properly made, the urine will flow through the wound without difficulty. The patient should be put on his left side, a layer of mackintosh under the buttock; the feet should be tied together, the thighs slightly flexed. The diet should be nutritious, and an opiate should be given at night-time if necessary. Cases for the most part do very well, unless there should be disease of internal organs, and more especially if the kidneys be affected.

It has been asserted that in the London hospitals the mortality from lithotomy is 1 in $4\frac{1}{20}$. Whether such a statement be true or not in the aggregate, it is hard to say. There may be incompetent operators or ill-ventilated or badly-arranged buildings. In St. Bartholomew's Hospital the mortality is about 1 in 10, nearly the same as in the days of Cheselden. I have had six consecutively successful cases, and others of my colleagues could give an equally favourable return.

I directed your attention to the case of a young man in Pitcairn ward, who is convalescent from the lateral operation performed nearly three weeks ago. You may remember that he suffered severe pain in micturition, and was liable to epileptic fits in rapid succession. The stone, when extracted, was found to be composed of oxalate of lime; it was as black and as nodulated, and in size the counterpart of a mulberry. Since the operation the lad has had but one slight epileptic seizure, and that during sleep. He is now so far recovered that I can scarcely persuade him to remain in bed.

As in the male, so in the female, I should say, Dilate, but do not divide more than is absolutely necessary. Those who have not tried will scarcely believe the extent to which the female urethra, the homotype of the membranous and prostatic portions of the urethra in the male, will yield to slow distension.

[During the last two years there have been thirteen lateral operations for stone, and of these two died, whilst out of the four lithotrity cases which have occurred, two died.]

Let us inquire into the causes of death in these two groups of cases. After the operation of lithotomy, of the two fatal cases, one, aged four years, died from diarrhœa; the other, aged sixty-three, from acute inflammation of a thickened bladder.

After the operation of lithotrity, of the two fatal cases, one, aged fifty-three years, died from bronchitis and abscess of the kidneys; the other, aged forty-seven years, from acute inflammation of the bladder, and granular degeneration of the kidneys.

These facts have been furnished me by the surgical registrar, Mr. Willett, whose accuracy and intelligence we all respect.

I grant that the data here given are not sufficiently large for permanent deductions; and yet they correspond with the general impression formed respecting the relative merit of the two operations—namely, that the dangers attending lithotrity are greater than are commonly supposed, and that the very favourable returns quoted by some surgeons proceed from their considering each "sitting" as a completed case; whereas the result should be regarded only after the patient has been freed of his complaint. The late Mr. Stanley was well aware of the dangers attending lithotrity; indeed he delivered a clinical lecture in this theatre in illustration of the fact. I believe that I express his opinion in saying that the operation of lithotrity is suited almost exclusively to cases of men in middle or advanced life, of gouty habit, and with a deep perineum, in whom, after intervals of a few months, a small uric acid calculus drops from the kidney into the bladder, the kidneys being in their general structure healthy. In such cases you may give the patient much relief and greatly prolong his life by crushing each uric acid calculus soon after it has dropped into the bladder, and while it is yet small.

The lithotrite also is useful in breaking up concretions formed around foreign bodies introduced into the bladder. From Ser. 36 (No. 196) I have selected this specimen. It consists of parts of a gutta percha bougie, about five inches in length, incrusted with deposits of lithate of ammonia, ejected from a man's bladder after being broken into several pieces by lithotrity. The bougie was being passed by the patient twenty-seven days

prior to its removal, when it broke between four and five inches from its distal extremity, the fragment being left in the bladder. A surgeon immediately attempted its removal by cutting into the urethra through the perineum, but a spasmodic action of the membranous portion ensued, and the whole fragment was drawn into the bladder. It there lay across the neck, was readily reached with the lithotrite, turned, and an effort made to withdraw it; subsequently I broke it into several pieces, each, as you see, about an inch in length. They were expelled at one act of micturition violently from the bladder the day following the last operation.

In this specimen (Ser. 36, No. 194) you see a portion of sealing-wax, which had been introduced into the bladder three years prior to its extraction. It is almost entirely incrusted with calculous matter.

The lithotrite should under no circumstances be used for the purpose of crushing an oxalate of lime calculus, the character of which is generally recognised by its sharp ringing sound. The fragments are always sharp and irritating to the bladder, and it not uncommonly happens that the lithotrite fails to act on the great mass of the stone.

Most calculi are composed of uric acid. We have a collection of above 200, and of these there are of uric acid alone, 30; of urate of ammonia, 13; of calculi composed of two layers, uric acid nucleus, 16; urate of ammonia nucleus, 25; of calculi composed of three layers, uric acid nucleus, 7; urate of ammonia, 18: total, 111. But this number would be much higher were it not that very many uric acid and urate of ammonia calculi have been thrown away as too small and too common for preservation. Next in frequency comes the oxalate of lime calculus, amounting to 40; of the fusible calculus, 19; phosphate of lime, 11; cystic oxide, 2. The other specimens are composed of carbonate of lime, or are concretions formed upon foreign bodies.—*Lancet, Dec.* 17 *and* 31, 1864, *pp.* 685, 739.

51.—THE PROOFS THAT LITHOTRITY IS AN EMINENTLY SUCCESSFUL OPERATION.

By Henry Thompson, Esq., Surgeon Extraordinary to H.M. the King of the Belgians, Surgeon to University College Hospital.

[An eminent London Surgeon (Mr. Coote) lately stated that "the general impression formed respecting the two operations," namely, lithotomy and lithotrity, is highly unfavourable to the latter, for "the dangers attending lithotrity are greater than are commonly supposed." The operation is moreover

suitable "almost exclusively to cases of men in middle or advanced life, of gouty habit, and with a deep perineum." Mr. Thompson differs from these opinions entirely.]

With the object of aiding the practical solution of this question, a group of consecutive cases of lithotrity will be presented, with their results. I shall, in short, adduce my own personal experience of the operation during the year just closed; that is, a brief statement of every case in which I have performed and completed lithotrity between Jan. 1st and Dec. 31st, 1864. And surely it will be admitted that, whether this *résumé* affords an exceptionally good result or not (and I contend that it does not), the possibility of such a result suffices to prove the extreme value of the operation for a very considerable range of cases. The patients on whom I have so operated during 1864 are nineteen in number; and I think the result may be regarded, although but a small contribution, yet as numerically much more valuable for inferential purposes than so small a number as four cases, of which two were fatal, which furnished the theme for Mr. Coote's remarks. As will appear, not one of mine was so; in one case only the operation was followed by death in a few weeks, opinions respecting the cause of which may be various; the others made good recoveries.

I ought to premise that, having personally no choice as to the performance of the cutting or of the crushing operation, I have been ready to apply either, according to what, after careful consideration of all the circumstances of the case, appeared to be the best for it. Lastly I have refused to operate in any way upon one case only—one in which the last chance of affording relief had wholly disappeared. Favourable cases, therefore, have not been selected for either proceeding. That there may be no error, I shall in each case name one individual who has seen it with me, so that a guarantee is afforded of the accuracy of my statement. Without any fear that it would be called in question, I still prefer this as the best rule to adopt, since it places the facts stated beyond the suspicion of error, and within the reach of present inquiry.

Case 1.—A gentleman, aged seventy-four. Uric-acid stone; medium size. Seven sittings. January and February, 1864; seen by Mr. Clover, who gave him chloroform at the patient's special request at first sitting, but was not repeated. Success perfect.

Case 2.—A gentleman, aged sixty-nine. Two uric acid stones of considerable size. Eighteen sittings. Spring, 1864. A patient of Mr. Gayland, of Devonshire-street, who was present on every occasion. Success perfect.

Case 3.—A gentleman, aged forty-five. Uric acid and urates; full medium size. Eight sittings. Spring, 1864. Dr. Koepl, of Brussels, who was then in town, was present at two sittings. Success perfect.

Case 4.—A gentleman, aged forty-six. Oxalate of lime; small medium size. Five sittings. Spring, 1864. Dr. Pancoast, of Philadelphia, then in town, was present at one or two of the sittings. Success perfect.

Case 5.—A gentleman, aged seventy-six. A smallish phosphatic stone. Five sittings. April and May, 1864. This case was seen by Dr. Jenner two or three times. Success perfect.

Case 6.—A gentleman, aged forty-eight. A medium-sized uric-acid stone. Six sittings. Mr. Hilton was present at one sitting. Success perfect.

Case 7.—A gentleman, aged seventy. Large mixed calculus. Eleven sittings. Summer, 1864. Seen two or three times by Mr. M. B. Hill. All symptoms of stone disappeared. Some trouble remains with pre-existing enlarged prostate; but he rides, drives, and is actively employed.

Case 8.—A gentleman, aged sixty-eight. A rather small calculus; mixed. Five sittings. Summer, 1864. Mr. Harris, of Gower-street, was present at every sitting. Success perfect.

Case 9.—A gentleman, aged forty-eight. A rather large uric-acid stone. Eleven sittings. Summer, 1864. Mr. Hilton was present two or three times. Success perfect.

Case 10.—A gentleman, aged sixty-nine. Small uric-acid stone. Five sittings; Dr. Koepl was present at the first. The symptoms disappeared, and he returned home. A fortnight afterwards the patient called on me to complain of a little return of pain. I advised waiting a week or two before sounding. This was at the beginning of June. At the end of the month he had severe rigors and fever, no instruments having been used. He gradually sank in the middle of July. At the autopsy a calculus, the size of a bean, and smaller ones, were found in the right kidney; in the left was a large abscess. In the bladder there were four calculi like those in the kidney, which had probably recently descended. The operation was incapable of removing the graver part of the malady—viz., that of the kidneys.

Case 11.—A gentleman, aged sixty-nine. Large uric-acid stone. Ten sittings. Summer, 1864. Mr. M. B. Hill saw this patient frequently; my friend Mr. Norman, of Southsea, also on one occasion. Success perfect.

Case 12.—A gentleman, aged sixty-two. Autumn, 1864. Small uric-acid stone. Three sittings. Mr. Hammerton was present each time. Success perfect.

Case 13.—A gentleman, aged sixty-two. Autumn, 1864.

Large calculus, uric acid and urates. Thirteen sittings. Dr. Harley was present at one sitting. Success perfect.

Case 14.—A gentleman, aged fifty-nine. Small uric-acid stone. One sitting. Mr. Clippingdale saw this case with me in Nov., 1864. A subsequent severe illness, from which he slowly recovered; but he writes me now that he has "no return of the old complaint."

Case 15.—A man, aged fifty-nine; University College Hospital. Rather small uric-acid stone. Eight sittings. Nov. and Dec., 1864. The calculus could never be found or seized in any other way than with the blades of the lithotrite directed downwards immediately behind the prostate, however high the pelvis was raised. Discharged free from all symptoms.

Case 16.—A boy, aged six; University College Hospital. Small uric-acid stone, the size of a small bean, crushed at one sitting. Discharged cured Dec., 1864.

Case 17.—A gentleman, aged seventy-five. A rather small phosphatic stone, broken up and removed at one sitting in Dec., 1864. Mr. Tatham, of Brighton, was present. The patient had long passed no urine, except by catheter. This condition of course remains, minus the symptoms of stone.

Case 18.—A gentleman, aged fifty-seven. A small oxalate-of-lime calculus, crushed at one sitting in Nov., 1864. Mr. M. B. Hill was present. Successful as far as the removal of the stone is concerned. The patient is still subject to the formation of small calculi, which he has passed in great number.

Case 19.—A gentleman, aged fifty-eight. A uric-acid stone of rather more than medium size. Five sittings. Dec., 1864. Mr. P. Jackson and Mr. C. Jackson, of Wimpole-street, were present at each sitting. Success perfect.

This list comprises eighteen adults, all relieved of stones, of which but a few were small. One patient only died, and certainly not as a direct result of lithotrity, but of renal disease and renal calculus, after having recovered the primary effects of the operation. Of these eighteen adults, the youngest was forty-five years old, the eldest seventy-five years; the majority were above sixty-two years; four were seventy and upwards; and the average age of the whole number is also, as nearly as possible, sixty-two years. No one, I presume will imagine that for these cases the operation of lithotomy would have afforded results in any way comparable; nevertheless many of the stones were of considerable size.

I venture to add that, in my opinion, the success of lithotrity depends upon the method adopted. During a considerable period of the history of that operation its results were undoubtedly inferior to the results of lithotomy in average hands; and if the same method be still employed, no better results can be expected.—*Lancet, Feb.* 25, 1865, *p.* 199.

52.—THE "MODUS OPERANDI" IN SUCCESSFUL LITHOTRITY.

By Henry Thompson, Esq., Surgeon Extraordinary to H.M. the King of the Belgians, Surgeon to University College Hospital.

[There is no operation in surgery which depends more on attention to minute details for its successful performance than does the operation of lithotrity. There are some leading general principles which direct its practice, which may be thus stated :—]

1. That mechanical contact with the bladder and urethra, whether from instruments or from rough fragments of calculus, is to be reduced to the smallest possible extent.

2. That the natural functions of the urinary organs are to be as much as possible rendered auxiliary to the operation; and that they are to be interfered with, and, *à fortiori*, to be impaired, as little as possible.

In illustration of the first principle—viz., to avoid all unnecessary instrumental contact with the bladder and urethra, I may say that in the nineteen cases related in the preceding article as having been operated on by me during the year 1864, and comprising 124 distinct sittings, not one preliminary injection was employed, nor was there a single washing out afterwards to remove débris. The lithotrite was rarely introduced more than once at a sitting, never more than twice, and no sitting exceeded five minutes in duration; the majority did not last three minutes. Lastly, a large fragment was never withdrawn by the lithotrite. By this practice a great amount of mechanical interference was spared to the patient. I think I have seen as much harm result from rapid and forcible distension of the bladder by injections for the removal of débris as from any other part of the operation of lithotrity, excepting always, it may be said, unnecessarily rough and frequent use of the lithotrite. The evils which follow rapid distension of the bladder in washing out forcibly are, chronic cystitis and atony of its coats. No doubt the former condition is often treated in the most efficient manner by washing out gently, and with small quantities of fluid slowly thrown in; but that is a kind of washing which is useless to remove débris.

On the same principle, I greatly prefer to avoid any unnecessary introduction of the lithotrite. The object has been to use an instrument which will pulverize the stone as much as possible, so that it may be ejected during the natural efforts of the bladder, and not to remove more débris in the blades of the lithotrite than can be easily withdrawn without encountering resistance, so as not to damage the neck of the bladder or the urethra.

The sitting, also, should be short: two minutes, for the presence of the lithotrite in the bladder, suffice for all ordinary purposes: sometimes three minutes are necessary. Ample time is thus afforded to seize and crush fragments four, five, or six times, employing the flat-bladed lithotrite with no opening in the female blade (so often miscalled or confounded with the "scoop," a wholly different instrument), and removing with it only as much as can be contained without causing difficulty in the withdrawal. Rarely should there be more than a slight appearance of blood, either at the time or in the first urine subsequently passed; often there will be none. The operator requires no other instrument with him than the lithotrite, and for so short and unirritating a proceeding he requires no chloroform and no preparation. The urine may have been voided half an hour or an hour previously; it is of small consequence, provided that too much is not present, a large quantity usually making it less easy to seize the stone. The patient soon loses his dread of a proceeding so little disturbing; and, as the sitting is rarely followed by a chill, it may be repeated every fourth or fifth, sometimes in exceptional cases every second or third day, without inconvenience. If more than ordinary soreness or irritability of the bladder is found on the introduction of the lithotrite, it is withdrawn, or there is a shorter sitting than usual, and so a chill may be avoided; conditions which under anæsthesia would not be recognised. Thus, it may be added, that in two only of the patients referred to (the single child's case of course excepted) was chloroform administered, and each of those at the first sitting only; the second was submitted to, at my request, without it, after which each patient preferred to dispense with chloroform, and had all the subsequent sittings without it. (Cases 1 and 12.)

The flat-bladed lithotrite, referred to above, has the singular merit of finely pulverizing the large fragments very easily and expeditiously; for the object is not only to avoid irritating the urethra, and especially the neck of the bladder, by withdrawing large calculous fragments with rough and sharp angles, but, in conformity to the first general principle, even to hinder such fragments from traversing the canal by themselves. When thus pulverized, the results of the sitting are easily passed by the performance of the natural functions, which, after the operation has been completed, even the elderly patient ought almost always to retain unimpaired. But when large fragments have been drawn through the neck of the bladder, retention of urine often follows, not to say cystitis, and the expelling function is often temporarily, sometimes permanently, impaired or even destroyed. After the first or second crushing of a full-sized stone, when it is necessary to use a powerful and fenestrated instru-

ment, large fragments must necessarily result, and very little fine débris indeed is made: but so far from desiring to remove these, I cannot but regard it as an important maxim to discourage them from passing at first. By keeping the patient in bed for twenty-four or thirty-six hours after the first sitting, desiring him to pass urine only in the recumbent posture, and, if he can, while lying on his back, irritation is reduced to a minimum; the fragments do not pass, but soon get slightly waterworn, their sharpest angles rounded, after which, if expelled, they pass much more easily than at first. The urethra itself has had time to recover from any soreness or irritation which it had acquired. Both the canal and the fragment are better adapted to each other than at first. I would rather see only a little débris pass until after the flat-bladed lithotrite has been used to pulverize the fragments, when the product will be expelled easily enough. On these principles, the rule is to interfere with Nature as little as possible. Nineteen-twentieths of all calculi are expelled by her without any help from us; for those of the remainder which come under the hands of the surgeon, she will aid us most efficiently and most safely if we will be content to trust her, and will not interfere to do roughly what she can do well and easily for herself. Time, a little more time, may be necessary to the process; but that is a small price to pay for increased safety to the patient. In this, as in other surgical proceedings, the *nimis diligentiæ* appears to be the especial source of numerous evils.

It would be affectation on my part to ignore the fact that the practice of removing fragments as little as possible by instrument is diametrically opposed to that of a well-known authority deservedly great on the subject of operations for stone; and, further that the system which Mr. Fergusson advocates of withdrawing many times during a single sitting large fragments from the bladder, is even regarded by some as an improvement in lithotrity. With great deference to one whose experience and opinions I, in common with my brethren, so highly respect, and from whom, indeed, I regret not a little to differ so widely, I feel compelled to protest against this view of that practice. I have no alternative but to say that increasing experience confirms me in my adherence to the principles laid down as the very foundation of successful lithotrity — viz., to diminish mechanical contact with the bladder and urethra as much as possible; and to crush so efficiently that the débris can come away easily by itself. At all events here is the fact, that, by a rigid adherence to these principles, nineteen consecutive cases of lithotrity, the majority patients of very advanced age, and some with large stones, were brought to a successful termination during the past year without that amount of instrumental inter-

ference which is very commonly employed. Further, in not one of these cases has the function of passing water been impaired by the proceedings adopted.—*Lancet, March* 4, 1865, *p.* 225.

53.—RESPECTING THE INSTRUMENTS EMPLOYED IN LITHOTRITY.

By Henry Thompson, Esq., Surgeon Extraordinary to H.M. the King of the Belgians, Surgeon to University College Hospital.

In order to reduce a calculus to the proper condition for passing from the bladder easily, two distinct varieties of instrument are required.

The first, a powerful instrument, of which the female blade has a large opening, sufficiently large to admit the male blade, and through which opening fragments are forced by the action of that blade. The second, a less powerful instrument, of which the female blade is wide, flat, much larger than the male blade, and without any opening to admit the latter.

The first, or fenestrated instrument, is necessary to the first crushing of any stone at all above medium size, and often to the second and third crushings if the stone is a large one; an instrument of the second class, with flat and non-fenestrated female blade, not being sufficiently powerful for the task. Now the fenestrated instrument requires considerable care in its employment; the edges of the male and female blade are sharp, and fit accurately to each other in order to insure an irresistible "bite" on the large hard stone. The result of its action on such a stone is the production of large, angular, wedge-like pieces, especially if the stone be composed of uric acid. Much space is also required for its employment; the blades must be opened widely in order to seize a large stone, and, from their own thickness, they project considerably beyond it; they must also have a length proportioned to the size of the stone. Being altogether a large instrument,—a condition necessary to the performance of its function,—it is not very readily moved in the bladder. Admirably adapted to the object —viz., that of reducing the entire calculus to several large fragments, it may be most advantageously replaced by the flat-bladed instrument when that object has been effected. To use it for reducing large fragments to smaller ones, is to employ an instrument which is not well adapted to that purpose, and one which is at the same time liable to produce an unnecessary amount of irritation.

For the purpose of reducing large fragments to smaller ones, or to débris, the flat-bladed instrument is greatly superior to

the preceding. It is a lighter instrument, and occupies less space; it can be opened to the requisite extent without impinging, so much as the fenestrated instrument, upon the walls of the bladder, especially at its neck. Its blades are shorter, it can easily be turned right or left, or rotated completely if need be. The edges of the blades are rounded, since nothing could be gained by making them sharp; they do not even meet each other, an interval always remaining between them when the instrument is tightly closed, so that a tyro need scarcely fear to lay hold of the mucous membrane. Its blades being wide and flat, not hollowed or scoop-like, and without an opening (except a very small one at the angle), the fragments are easily seized, do not readily slip, and when crushed are reduced mainly to powder between two flat opposing surfaces. For the same reason, if the urethra is capacious, the instrument may be withdrawn retaining a good deal of this powder (not sharp fragments) impacted between the blades; while if the urethra is not capacious, it is easy to disengage the greater part of this powder from the blades before withdrawing. As a rule, such an instrument may be used, I venture to say, with advantage, certainly seven times as often as the fenestrated instrument. In the nineteen cases I have reported, with 124 sittings, certainly not less than 100 to 110 were conducted with the flat-bladed lithotrite; indeed the other was only used for the larger stones. But the flat-bladed instrument may be constructed on a very powerful model. If the male blade is made about half the width of the female blade, and with a very slightly wedge-shaped contour, very slight indeed, as I have suggested and employed, it will readily penetrate any uric-acid stone of an inch or an inch and a quarter in diameter. It should here be said that if the male blade has much of the wedge-shaped form, like a ∧, for example, it not only cuts quickly into the calculus presented to it, but throws off the fragments laterally with extreme force. I tried the experiment some years ago, and was struck with the dangerous facility with which this happened: such fragments were thrown several feet from the instrument when air was the surrounding medium, and very far in water—a totally different result to that from fracture by the ordinary square male blade. The very slight wedge form above described has also the advantage of obviating impaction of the blades with débris, which is more likely to occur with a flat-bladed instrument in proportion as its size is augmented.

Nothing can be more distinct than the characters of the true flat-bladed lithotrite and those of the instrument which has long been termed a "scoop." The latter was introduced into practice long before the flat-bladed lithotrite was made. In the scoop the female blade is much hollowed out, as the name

implies, and the edges of both blades fit accurately one to the other: for this reason there are strong objections to the instrument; and, further, because its hollowed blade is necessarily easily impacted by débris; because a small quantity of débris largely augments the calibre of the blades; and, lastly, because this débris cannot be dislodged after impaction has taken place.

Next, in order to reduce mechanical contact with the bladder to its minimum, it is essential to employ a lithotrite of which the two motions, those of the screw and the sliding movement of the male blade, are easily exchanged the one for the other. The lithotrite which has long been used in this country—always by Sir B. Brodie—is one in which the blades cannot be reopened after having been screwed home, except by the tedious act of unscrewing. Professor Fergusson overcame this great defect by substituting the rack and pinion for the screw as the mechanical power. Speaking generally, however, the screw is still much preferred to the rack and pinion, now that its action can be instantly detached, and its blades opened and closed by a sliding action convertible into screw action by a simple movement of the thumb. Regarding carefully all the points of importance, I do not know any instrument, British or foreign, which at this moment so completely supplies all the desiderata as a form of lithotrite which has been recently made by Messrs. Weiss: that is, one which does its work with so small an amount of concussion in the bladder. It can be turned smoothly round with the greatest facility, merely by the finger and thumb of the left hand, in prosecuting the most delicate search; it can be held immovable with the firmest grasp by its cylindrical handle when dealing with a large stone. I have long felt the want of such a handle in the French lithotrite, which up to three years ago appeared to many to be the most commodious instrument, and in consequence suggested the long cylindrical one; and it has answered beyond expectation. The mode in which the sliding action is converted into screw action, and *vice versâ*, which Messrs. Weiss have designed and applied, is much simpler, easier to accomplish, and less liable to cause lateral movement of any portion of the instrument within the bladder, than is the French instrument.

So much for lithotrites. With regard to washing out the bladder for the removal of débris, I am sure all who have much employed it must confess how little productive is the process. When much débris is within the bladder, no doubt it is easy enough to remove some of it thus; but then no difficulty exists respecting this débris. It will pass off by itself readily enough. It is the obstinate fragment at last, or the two or three fragments, that do not come away, but remain in spite of repeated washings, which sometimes demand interference for their

removal, and may occasion a little trouble. Some accidental shape perhaps causes this: the fragment may be a flat scale perhaps, which adheres to the bladder, and is not easily transmitted by the natural current, or washed through the eye of a catheter. Whatever the cause, however, so it sometimes happens. For myself, I have preferred in such a case to detect its exact situation in the bladder by means of a small sound delicately used, and, having done so, the patient not moving, then to pass a small flat-bladed lithotrite straight to the spot, when three times out of four it may be picked up at once, usually with the blades pointing downwards, and found often immediately behind the neck of the bladder.

I have recently seen, and two or three times used by way of experiment, an instrument for removing débris from the bladder, designed by Mr. Clover, which promises to be useful. He has contrived a very ingenious method of making a current of not more than one or two ounces of water pass to and fro from the bladder into a glass receiver, into which the débris is deposited. Mr. Clover will, no doubt, describe its action before long. It seems to me much more efficient than the recent application of the ancient method of irrigation by means of the screw, which has been employed by M. Maisonneuve, and one of which instruments I have.—*Lancet, March* 11, 1865, *p.* 254.

54.—THE PECULIAR APPLICABILITY OF LITHOTRITY TO CASES OF SO-CALLED "PARALYSED BLADDER."

By Henry Thompson, Esq., Surgeon Extraordinary to H.M. the King of the Belgians, Surgeon to University College Hospital.

[Formerly it was the custom to regard as contra-indicating the performance of lithotrity that condition of the bladder in which the patient has lost the power of passing any urine whatever by the natural efforts. This loss of power in most instances is due not to any impairment of the nervous supply to the organ but to some obstructive disease of the urethra such as prostatic enlargement, impeding the action of an otherwise healthy bladder.]

In some cases, again, it may be due to loss of contractile power in the muscular coat of the bladder, produced by some accidental, perhaps voluntary, over-distension—in other words, to atony of that viscus; no obstruction existing in the course of the urethra. And, lastly, these conditions may co-exist to a greater or less extent in any given case.

I have for some time been led to regard such a condition of the bladder not merely as one in which lithotrity ought not to

be performed, or might be performed with difficulty; but, on the contrary, as one which is peculiarly favourable to the success of the operation. I have no hesitation in regarding it as presenting circumstances more likely to conduce to a happy result than those which are presented by the ordinary or average run of calculous patients in middle life who have perfect command of the urinary functions. Nevertheless, examples of success in the cases referred to are still sometimes spoken of as unusual or exceptional instances. I ventured to express the opinion given above in the Lettsomian lectures of 1862, and I am no less convinced of its correctness now. The reason seems to be this, that in cases of patients who have been compelled to use their catheters several times daily for some months or years, the passages have become so accustomed to instruments that the pulverized débris may be withdrawn by the lithotrite with comparative impunity; and in these I do not hesitate to do so, still avoiding the removal of fragments which would irritate the urethra and occasion bleeding. I have operated on several such cases, and have had none that gave less trouble or made better recoveries. It is rare for such patients to suffer from rigor or other constitutional disturbance, for the passages are callous to those sources of irritation which in the patient of ordinary susceptibilities, and unaccustomed to the employment of any instrument in the urethra or bladder, so often arouse systemic derangements of a serious nature.

I believe that it is an error to regard the small calculus recently descended from the kidney into the bladder of an apparently healthy middle-aged man as a matter of very slight gravity and of small surgical importance. I have occasionally seen more distressing and prolonged illness arise from the crushing of such a stone, although only one short and easy sitting sufficed to readily accomplish the object—than from stones of ten times the size. The very presence of a stone in the bladder for a few months, irritating as it is, seems sometimes to render the bladder accustomed to mechanical contact; and the lithotrite may sometimes be used more safely in such circumstances than in a bladder which has but recently become tenanted by a stone, or which has never been entered by a catheter or sound. The cases of small and recent calculus are those which, perhaps as much as any, benefit prospectively by passing bougies a few times preparatory to the subsequent use of the lithotrite. With all the usual precautions the case of small calculus becomes one of extremely favourable augury; but it is hazardous to be in the least degree indifferent to precautions because the calculus is small or recent.

I have operated in seven cases in which the patients had for years depended on the catheter to evacuate the bladder: in

one instance from real spinal paralysis; in the other six, from atony of the bladder or from enlarged prostate. One only ever had rigors; all made excellent recoveries.—*Lancet, March* 18, 1865, *p.* 281.

55.—OBSERVATIONS ON LITHOTOMY.

By James R. Lane, Esq., Surgeon to St. Mary's and the Lock Hospitals, and to St. Mary's Hospital for Fistula, &c.

[The method of operating recommended by Mr. Lane is based upon that introduced and long successfully practised by the late Mr. Aston Key, with a straight staff. Mr. Key's objection to the ordinary curved staff was that it is of the form least adapted to convey a cutting instrument where the eye of the operator cannot follow it.]

The advantage of having a straight director for the knife has always appeared to me to be very great. The pattern of the staff used by Mr. Key is, however, in my opinion, faulty in some important particulars. In the first place, it is not sufficiently curved at its extremity, so that there is still risk that in the hands of an indifferent assistant its point may escape from the bladder; secondly, the groove is continued beyond the straight part into the bend, which circumstance renders it possible that the point of the knife may pass out at the end of the groove, and even penetrate the opposite side of the bladder — an accident which I have been told has actually occurred. The first disadvantage may be avoided by having the last inch or inch and a quarter of the staff abruptly curved, so as to resemble in form the ordinary lithotrite; it will then not be likely to slip out of the bladder, and it will be of the most convenient form for discovering the stone before the operation is commenced. The second risk is avoided by having the groove made to terminate just before the curve begins, and to terminate, not gradually, but by an abrupt stop, against which the point of the knife may be distinctly felt to strike. The curved part of the instrument should be regarded in the light of a sound; the straight part, and that only, as a director to guide the knife into the bladder. During the first part of the operation the staff is held by an assistant, not perpendicularly, as when the usual curved instrument is used, but inclined at an angle of 45° with the patient's body. The superficial incisions having been made in the usual way, the urethra having been opened, and the point of the knife securely lodged in the groove, the operator takes the handle of the staff in his left hand, and depresses it nearly to the horizontal line before he commences his deep incision. At the same moment, holding

the knife firmly in the groove of the staff, he turns the groove, which has hitherto been directed downwards, obliquely towards the left side of the patient, and thus lateralizes both knife and staff to the degree required for the section of the prostate gland. He then pushes the knife steadily onwards until it is arrested at the end of the groove, or until a copious gush of urine assures him that he has penetrated far enough. It is almost impossible that the knife should escape from the groove; and by holding the staff in his own hand while he is using the knife, the operator will have a feeling of consent and security in the manipulation of the two instruments, which can never be obtained while one of them is in the hands of another individual. By firmly depressing the handle of the staff, its point, and with it the prostate and neck of the bladder, are raised in a corresponding degree, and are lifted away from the rectum, the danger of wounding which is consequently diminished. Still further safety to surrounding parts may be obtained by the operator pressing his knife firmly against the staff while he is making his incision, so as to carry both staff and prostate upwards and towards the right pubic ramus. He then withdraws and relinquishes the knife, and takes hold of the staff with his right hand, while he passes his left forefinger through the wound along the staff into the bladder. Having ascertained in this way that the incision has been satisfactorily completed, and having felt the stone, he withdraws the staff, and exchanges it for the forceps.

The advantages of the straight over the curved staff in making the deep incision are great; it enables the operator to take the staff in his own hand, his left forefinger not being required in the wound to protect the rectum, or to direct the passage of the knife; it admits of being lateralized together with the knife during the deep incision; and in the most important and delicate part of the operation, the knife, while out of sight, has to move along a straight instead of a curved instrument as a director. The sole objection to it that I can see is that it does not afford the same support to the first incision, and that the groove is not quite so easily exposed. It carries the urethra further from the surface, and there is a probability that it may be opened too far forwards, and that the bulb may be wounded. A very little care and practice will enable the surgeon, by directing the point of his knife somewhat deeply, and aiming at the apex of the prostate at the moment of opening the urethra, to avoid this error. I cannot conceive that anyone who is competent to undertake lithotomy at all, can have any appreciable difficulty in safely completing this stage of the operation.

In making the first incisions in lithotomy nothing is gained

by commencing too far forwards. No more room is thus obtained for the extraction of the stone, while the liability to wound the bulb or its artery is increased. In my opinion the distance of an inch and three-quarters from the anus, sometimes recommended, is more than is necessary. I should say an inch and a quarter in an adult, and an inch in a child, afforded amply sufficient room in this direction. Some have recommended the incision to be commenced some distance to the left of the raphé, in order to avoid the bulb; it is quite true that by so doing the danger to the bulb is diminished, but the danger to its artery, which is of much more importance, is proportionately increased. The artery of the bulb passes obliquely upwards and inwards beneath the triangular ligament to enter the bulb upon its upper surface. Its position varies a little according to the level at which it takes its origin from the internal pudic, but its average course is parallel to, and a little in front of, the transversus perinei muscle. A due appreciation of this fact is of great practical value, for if the incision be begun away from the median line, and be continued deeply above the level of the transversus perinei, this artery is nearly certain to be cut across. I always commence the incision close to the raphé, and by doing so I believe the artery of the bulb is in almost absolute safety, while the bulb itself, which, however, is of less importance, may be avoided by not inserting the knife too deeply at the commencement of the first incision, and by aiming behind the bulb towards the apex of the prostate at the moment of entering the urethra.

A few years since (see the 'Lancet' for 1856 and 1857) an animated discussion took place as to the mode of holding the knife in the deep incision adopted by the late Mr. Liston, who seems to be generally accepted as the model lithotomist of modern times. In the drawing given in Mr. Liston's book, which has been followed in the earlier editions of Messrs. Miller, Pirrie, and Erichsen, the knife is placed over the hand—that is, it rests upon the palm, which is directed upwards, and the forefinger is advanced to support the blade, and steady it against the staff. According to Mr. Syme, Mr. Liston afterwards had the posterior part of the blade blunted, like the knife now in ordinary use, in order that the finger might not be injured. This, however, is not represented in the drawing. Mr. Fergusson maintains that there has been a mistake on the part of the artist; and that Mr. Liston was not in the habit of holding the knife in the manner represented, but *under* the hand, with the forefinger resting upon the back of the blade, which is the method adopted by himself. Mr. Fergusson's views are supported by Mr. Miller and Mr. Cadge, both of whom had ample opportunities of witnessing Mr. Liston's opera-

tions; while, on the other side, Mr. Syme, who is corroborated by Mr. Pirrie, positively asserts that Mr. Liston's practice was to hold the knife over the hand, supported by the forefinger, and that he "would have been horrified at the idea of running a knife into the pelvic viscera of a patient without confining it it between the finger and a director."

I am, of course, unable to throw any light upon this controversy: I allude to it merely as exemplifying the importance attached to the point by several of the leading surgeons of the day, and for the purpose of adding that the method which instinctively recommends itself to me is to hold the knife *under* the hand, with the forefinger on the back of the blade, during the first part of the operation, and until the urethra is opened, but to shift the hand into the position attributed to Mr. Liston by Mr. Syme, and represented in Mr. Liston's diagram, before commencing the deep incision. It appears to me that a greater degree of security, with equal freedom of movement in making the deep incision, is in this way obtained.

Since I adopted the plan of operation with the straight staff herein described I have operated on ten cases of stone. Eight of these have occurred at St. Mary's Hospital; all have been children under ten years of age, and all have recovered without the intervention of a single unpleasant symptom. Of the other two cases, one was a child of four years, under the care of Dr. Baber, of Brompton, which also recovered favourably; the other, which terminated fatally, was a patient of advanced age, under the care of the same gentleman, in whom, however, the operation was undertaken almost as a last resource, to obtain relief from severe suffering, and without very sanguine hopes of ultimate success. In this last case I found the straight staff answer remarkably well. The deep incision was completed with the greatest ease, although the prostate was of large size and the perineum deep, so much so that the bladder could not be afterwards reached with the finger.

In the nine children alluded to I never encountered the least difficulty or delay in cutting into the bladder, except to a slight extent on the last occasion, in which a second application of the knife was required, the incision not having been carried far enough at first. This case, I explained to the students at the time, afforded a good exemplification of the difficulty which may arise in children, owing to the bladder being situated higher in the pelvis, and therefore deeper from the surface of the perineum, than in the adult; and, perhaps in a still greater degree, to the looseness of its connexion with surrounding parts, and consequent greater mobility. I found that the incision first made would not permit the finger to enter the bladder, and that the slightest attempt to force it along

the staff in that direction only caused the bladder to recede before it without any nearer approach to the object in view. I of course immediately desisted, and, having introduced the knife again into the groove, passed it onwards until the bladder was fairly opened. Nothing would have been easier in this case than to have excavated with the finger a space in the recto-vesical region, which might have been easily mistaken for the bladder; or, with a greater degree of force, to have detached the bladder completely from the urethra, and to have penetrated into the cellular interval between the bladder and the pubic symphysis. No doubt this is the true explanation of the failures to complete the operation in children which are occasionally heard of, where no stone has been discovered after a protracted search; but in the cases where the patient has had the good fortune to recover, one has been removed by a subsequent procedure. The bladder has, in fact, never been opened at all, but a cavity, which has been mistaken for the bladder, has been hollowed out by the operator's fingers in the perineum. The knowledge of this cause of failure is no new thing to those experienced in lithotomy; nevertheless it has not always been noticed by surgical writers so prominently as it deserves, and it is impossible that the caution against it can be too often repeated. Those unaccustomed to lithotomy in children are often insufficiently aware of the great depth to which it may be necessary to carry the incision, and of the facility with which the mishap here alluded to may occur to the unwary. Mr. Fergusson has recently dwelt very forcibly on this subject, in his lectures delivered at the College of Surgeons. (See the 'Lancet', July 2nd, 1864.)

Children with stone in the bladder are, as everyone knows, very liable to prolapse of the rectum, and the bowel is frequently protruded just before the operation, during the struggles incident to the inhalation of chloroform. Under these circumstances I have often seen it returned by the surgeon before commencing the operation. I believe, however, that this is a mistake, and that it is better to allow it to remain down. If returned, it is lodged in a mass just within the sphincter, and is in danger of being wounded, as it encroaches on the space required for the incisions. I have on several occasions allowed it to remain down throughout the operation, and have never found any inconvenience from so doing.

In the present day, when the operation of lithotomy has been decided upon, the question arises whether the median operation, about which so much has been said of late, should be adopted in preference to the old lateral method. The median operation —the essential features of which are, a limited incision of the urethra in the median line of the perineum, and forcible dilata-

tion of the prostate and neck of the bladder--has, as is well known, been much advocated of late years; it has been adopted by various surgeons of repute, and has been attended with a considerable degree of success. The success, however, of lateral lithotomy in children is so well established that the median operation can scarcely surpass it in this respect; while in adults, if we may judge from recent statistics, the inferiority of the new method becomes evident. I allude to the tables published by Mr. Williams, of the Norfolk and Norwich Hospital, of forty-four cases of median lithotomy, compared with the last forty-four current cases of lateral lithotomy performed in that institution up to November, 1863. So far as the numbers go, this appears to be as fair a mode of comparison as could be devised. There were eleven deaths after the median, only two after the lateral operation. The patients were of all ages, but the median operation had the advantage of a rather larger proportion of children. There was one case of death after the median under five years of age, but the principal mortality occurred between the ages of sixty and seventy. The weight of the stone is given in each instance: in no case except one did recovery take place after the median operation when the calculus exceeded in weight three drachms two scruples. It is right, however, to mention that the period required for recovery was found to be less after the median than after the lateral operation; the average time being thirty days in the former, and thirty-seven days in the latter.

From this it may be inferred that median lithotomy in children is not safer than the old operation, while in adults it is not so safe; that in either case, if the stone is large, it is decidedly the more dangerous of the two, and therefore should only be practised, if at all, where there are sufficient grounds for believing the stone to be of small size; and that its principal recommendation is the somewhat shorter period required for recovery. I have never yet felt disposed to adopt it, not having been able to see that anything is gained by making the incision in the limited space which is available in front of the anus, rather than in the much more convenient and spacious lateral region; and being far from convinced of the advantage of forcible dilatation and laceration of the prostate and neck of the bladder over the limited incision of those parts, now generally practised in the lateral operation.—*Lancet, Feb.* 11, 1865, *p.* 142.

56.—ON THE SOLUTION OF CALCULI IN THE BLADDER AND KIDNEY.

By Dr. W. Roberts, Manchester.

[A very interesting paper was read on the above subject at a

meeting of the Royal Medical and Chirurgical Society. The treatment recommended is only applicable when the calculus is small and of the urid acid kind, and when the urine is not alkaline from disease.]

His treatment consists in the administration of large doses of acetate or citrate of potass, so as to keep the urine constantly of a certain degree of alkalinity. For weeks together, the patient may have his urine thus kept alkaline without injury. Dr. Roberts, in fact, considers the administration of alkalies in this way as harmless as the taking of so much sugar. The paper was remarkable as a methodical, scientific study; and, also, well indicates the immense service which we may anticipate that chemistry will one day render to medicine. The real value of the proposed remedy can of course only be decided by future experience.—*British Medical Journal, April* 1, 1865, *p.* 326.

57.—ON THE CURE OF HYDROCELE BY ACUPUNCTURE.

By Dr. John Thompson, Bideford.

[Acupuncture is not unfrequently attended with a radical cure of the disease.]

In the congenital hydrocele of children, it will very generally succeed, and not unfrequently in that of adults. I admit its great inferiority to the practice by injection; but where this cannot be adopted, as in children and old persons, it seems to me but a common-sense thing to use the method next in order of efficacy.

For many years, I have not tapped a hydrocele with a trocar, unless as a prelude to injection; but have, in all cases, used acupuncture, and, to my great gratification, every now and then a cure is effected, with less inconvenience to the patient than had he been tapped as many times in the ordinary way.

It is no new suggestion to treat hydrocele thus, as it was long ago tried in Germany, and since in this country by Mr. R. Keates and Mr. Travers, who reported that "it acted favourably in a number of cases, but was not generally to be relied on."

I believe this is quite as much as can be said for it. My position is simply, that, as acupuncture properly performed gives the patient even less inconvenience than tapping with the trocar, and is frequently followed by a radical cure, which the operation by tapping is not, therefore it is advisable to use acupuncture in all those cases that are unsuitable for the treatment by injection, seton, &c.

There is something to be said about the manner in which acupuncture is to be performed. It is advised by a recent writer

to use a grooved needle, but for what reason, I cannot understand. If the needle is cylindrical in the shaft, with a conical point, as is usually the case, it is a very unscientific shape for puncturing tight structures like the skin and sac. All needles intended to penetrate the human structures readily, should be either triangular or spear-shaped at the puncturing extremity; and the shaft should not be large. I always use a needle that Mr. Liston employed in the operation for hare-lip; it has an expanded spear-shaped extremity for piercing, and a small cylindrical shaft. From three to six punctures may be made with this, well through the skin and sac, the scrotum being grasped firmly on its hinder part in the left hand at the same time, so as to tighten the integuments. No marked pain is felt; a little fluid oozes from the punctures while the hand is applied, but ceases on its removal; in a day or two the tumour is gone.

The needle used by the sewers of kid gloves will answer equally well with that I have described, it has a sharp triangular extremity for piercing; our needle-makers knowing that this is the form necessary for entering tough animal structures, the round conically pointed needle is only applicable for loose textures.—*British Medical Journal, Oct.* 22, 1864, *p.* 465.

58.—ON THE TREATMENT OF HYDROCELE.

By Dr. William Jollie, Gateshead-on-Tyne.

[Dr. Jollie recommends the following plan which he has followed for some years, and invariably with success.]

I tap the hydrocele by trocar and canula in the usual way, draw off the fluid, and then introduce through the canula into the cavity of the tunica vaginalis a common surgeon's probe, which has been previously coated for an inch of its length with nitrate of silver. I prepare the probe by heating the extremity to a dull red heat in the flame of a gas-light, and placing it in a little finely-powdered nitrate of silver, and then again subjecting the probe to the heat, so as to form a smooth coating to the instrument. If your correspondent and other surgeons will make use of this method, they will, I have no doubt, quickly, effectually, and cheaply relieve their patients of a troublesome complaint.—*Lancet, Jan.*14, 1865, *p.* 54.

59.—ON A CASE OF DEATH RAPIDLY FOLLOWING THE USE OF HOLT'S DILATOR.

By Henry Smith, Esq., Assistant-Surgeon to King's College Hospital.

William H., aged twenty-four, single, a clerk, was admitted into King's College Hospital under the care of Mr. H. Smith,

on Feb. 8th, for stricture. He had suffered from it for three years, and had been under various surgeons. During the last nine months his symptoms had increased, and he applied at the hospital on the 3rd of February, when the house-surgeon attempted to pass an instrument, but failing, sent him to Mr. H. Smith, who, after a great deal of difficulty, succeeded in introducing a No. 3 silver catheter into the bladder. There was a very tight stricture at the anterior part of the bulb, and, after having passed through the stricture, the point of the catheter diverged towards the left side, and was with difficulty made to find the right passage. Finding that the stricture was so tight, and that the patient was anxious to get well rapidly, Mr. H. Smith admitted him into the hospital. The patient in every other respect was a remarkably healthy man. On admission, Mr. Smith passed a No. 3 silver catheter, but with extreme difficulty. This was tied in the urethra.

Feb. 10th. The catheter keeps in the bladder, but he has had a rigor. In consequence of this the same catheter was retained, and he was ordered one grain of opium powder and two grains of quinine three times a day.

11th. No more rigors. At three p.m. to-day the patient was placed under the influence of chloroform, and a Holt's dilator was readily introduced, the largest tube passing down without difficulty. The stricture was split, a No. 11 catheter passed into the bladder, and the urine drawn off, scarcely any blood being mixed with it. The patient was placed in bed, and ordered to take the opium and quinine. He felt so well when he returned to bed that he was almost disposed to doubt that anything had been done to him.

12th. He has had a good night, but passed no water until eight this morning. Very soon after this he had a most severe rigor, followed by vomiting. From this time until one p.m. he passed a considerable quantity of water highly charged with blood. At four p.m. he became very collapsed, blue in the face, and slightly delirious. He was ordered brandy, and hot turpentine stupes to the chest and loins.

13th. He has remained in much the same condition during the night. At eight a.m. the house-surgeon passed a catheter into the bladder, but only drew off a little blood. He gradually got worse during the day, and died at half-past four p.m.

The friends objected to a post-mortem examination, and therefore only the urethra and bladder taken out through the perineum could be examined. The urethra was found to be full of blood, and the strictured portion, which was in the bulb, was seen to be split by the dilator; but there was no laceration of the healthy portion of the canal, and there was no extravasation of urine. There was no examination of the kidneys

possible. The bladder was small and contracted. A false passage existed in the urethra, beginning in the left, just in front of the stricture, and running towards the right, not opening into the bladder.

This case presents features of gave interest, especially at a period when the treatment of stricture by rapid dilatation is being tested to a great extent, and I think it would be highly improper not to make the case public. The sudden and mysterious death following on an operation, unattended with the slightest difficulty, cannot fail to arrest the attention of even the most careless; and those of us who are too self-reliant are taught by it the humiliating lesson that disastrous results will follow those efforts which would appear most likely to be crowned with success, and least liable to failure. Here is presented to us all the conditions for success. A young man in remarkably good health, the stricture not complicated in any way, and not the least difficulty in performing the operation; careful watching afterwards, together with attentive medical treatment; and yet this man is completely killed, solely and entirely, as it were, by the shock of the operation, through it, by the impression made upon the system. That most terrible of all symptoms in the urinary organs—viz., suppression of urine, coming on a few hours after the operation, rendered the case hopeless, in my apprehension, from the first, and doubtless this was the condition which led to the patient's destruction; and although we know that it is a rare accident, still we must not for the future, in calculating the dangers of this operation, lose sight of the possibility of such an occurrence.

I regret to state that this makes the third case in which fatal results have followed upon this operation at King's College Hospital; and I am obliged to state that, so far as I have witnessed the execution of it in the hands of my colleagues and of myself, the proceeding has always been accomplished with great ease and strikingly little bloodshed or violence.

It is but right, however, on the other hand, to state that the results of my friend Mr. Holt have been wonderfully successful. He has informed me that he has had only one death in three hundred operations, and I need not say that the greatest possible reliance may be placed upon that gentleman's word. I do not believe for a moment that he would conceal a single fatal case; therefore, those who might be inclined to underrate the success of this operation from the occurrences in my hands and and in those of my colleagues must place against this bad fortune the extraordinary results produced by Mr. Holt. My friend Dr. Wiblin also informs me that he operated upon twenty-eight patients without a single bad result following, and was astonished when I told him of the disastrous event above

recorded. I may mention that I have seen, and indeed recorded, as suddenly a fatal result after the simple passing of a catheter; and we know that sometimes the process of sounding or searching with a lithotrite has been followed by speedy death. These facts should be borne in mind by us whilst endeavouring to estimate aright such an occurrence as I have thought it my duty to record.—*Lancet, March* 18, 1865, *p.* 284.

60.—THE UTILITY OF THE ENDOSCOPE AS AN AID IN THE DIAGNOSIS AND TREATMENT OF DISEASE.

By Dr. Francis Richard Cruise, one of the Medical Officers of the Mater Misericordiæ Hospital; and Lecturer on Medicine in the Carmichael School, Dublin.

[The endoscope is an instrument for the direct exploration of internal organs by the sight. There is no portion of the human body into which a straight tube can be introduced in which it will not be found to be of service. The interior of the bladder, urethra, rectum, uterus, nasal fossæ, pharynx, larynx, and even in time, perhaps the œsophagus and stomach. It is, however, chiefly of use in diseases of the urethra. The following is a description of the instrument.]

In the first place, there is a tube or speculum, which is introduced into the cavity to be examined; and at one extremity of this a mirror of polished silver, perforated in the centre, is placed at an angle of 45°. The function of the mirror is to reflect the light, which is placed laterally, into the tube, so as to illuminate it to the end. As the calibre of the tube is very small, a most brilliant light is required, and, in order to obtain the best effects, it should be made to converge slightly upon the mirror. This convergence is attained by interposing between the light and mirror a plano-convex lens of suitable focal length.

The light being sufficient, the lens properly adjusted, the mirror bright and correctly placed with respect to the tube, it becomes a matter of facility for the eye of the observer, looking through the perforation in the mirror, to see clearly to the bottom of the speculum.

Such is the endoscope I have adopted, in principle identical with that used in 1824 by Fisher of Boston.

To obtain a suitable light is *the grand difficulty* in the construction of the instrument, and it has cost me no inconsiderable labour to overcome this obstacle. Even at the end of my prolonged experiments I am compelled to acknowledge that the discovery of the illuminating power I have selected was to a great extent fortuitous.

Experiments which I need not recall here, but which are familiar to those who have made investigations with, polarized light, led me to the knowledge that one of the brightest illuminations which can be obtained by any means is that given off by the *thin edge of the flat flame* of a petroleum lamp. Moreover, the steadiness and intensity of the light are increased to the utmost by using a tall draught chimney, and by dissolving camphor in the petroleum, in the proportion of ten grains to each fluid ounce. I may further observe, that this light possesses the advantages of cheapness, freedom from danger, and total independence of such care in operation as to demand the aid of an assistant specially devoted to it. Similar attractions do not exist for the magnesium wire, oxycalcium or Drummond lights, or for that produced by electricity, which latter was proposed forty years ago for Fisher's apparatus by Professor Patterson.

Such then is the light I use and recommend, and I believe it will be found applicable to many other instruments. It is open, undoubtedly, to two objections. First, that it produces considerable heat; and secondly, the pencil of rays being extremely narrow, a very perfect adjusting apparatus is required to facilitate the movements of the lens and inclined mirror, so as to take advantage of it under all circumstances.

To obtain the best effect from the light a few precautions are requisite. The room in which the examination is made, so far as may be convenient, should be darkened; the lantern must be held steadily vertical whatsoever the position of the exploring tube may be, and the whole interior of the lantern and specula must be kept carefully blackened. I find that the best varnish for this purpose is made by adding a few drops of spirits of turpentine to some lampblack, procured by holding a piece of metal plate over a candle. Thus made, the varnish may be laid on with a camel hair brush; it dries rapidly, leaving a dull black surface which reflects little or no light.

Besides a good illuminating power, we require for the endoscope a variety of specula, adapted to the exploration of different regions of the body.

For general use the urethral tube can hardly be excelled. It consists of a narrow portion, the size of a large catheter, which is just six inches in length; the remainder gradually dilates to form the part which fits into the receiving socket, wherein it is secured by means of a thumb-screw.

A wire stillete surmounted by a plug is provided, which can be inserted into the tube in order to facilitate its introduction into narrow canals such as the urethra. At one side there is an opening wide above and narrow below, intended to admit probes, carrying either cotton wadding or sponge to wipe the parts under

examination, or caustic or other applications for the purposes of treatment.

Three or four sizes of these urethral tubes are required. They answer remarkably well for other situations, such as the uterine cavity, nasal fossæ, &c. By the aid of one I was lately enabled to ascertain, in a patient of my own, the precise attachment of a huge nasal polypus. In another case of obstructed nostrils, which Dr. Stokes requested me to examine, I was able with such a tube to exhibit a quantity of small gelatinous polypi, situated high up and far back out of sight, which produced all the annoyance.

For the exploration of the rectum a tube eight or nine inches long and more than half-an-inch in diameter may be provided. With such an instrument Mr. Connolly was able to make a drawing of a stricture of the rectum in a patient now under my care. For the examination of the external meatus of the ear and membrana tympani, and for the performance of operations thereon, I have had an auriscope made. Already I have found it most useful. Although not comparable to a direct ray of sunlight, yet, in its absence it makes a reasonably good substitute, and the oblong lateral aperture allows of the introduction of suitable instruments, which can be manipulated under the eye of the examiner. During the early part of the past winter I was baffled from day to day by the darkness of the weather in my efforts to obtain a view of the membrana tympani in a lady whom I attended for a sudden attack of deafness. At last I used a rude ear speculum, fashioned at the time to suit my endoscope, and at once obtained a satisfactory view of the parts. Since then I have frequently used my auriscope with the most satisfactory results.

So far as my experience goes to carry out that of Desormeaux, the most useful field for the operation of the endoscope is the urethra. By its aid diseases of this part, otherwise merely subjects of conjecture and empiricism, are rendered as clear as to diagnosis and as satisfactory respecting treatment, as affections of the eye, or of any other external portion of the body patent to view. Before entering upon the study of the diseases of the urethra, I may premise a few words about the operation of examining it endoscopically.

If the urethra is free from constriction, which can easily be ascertained with a bougie, the best course is to examine it from the neck of the bladder to the glans penis. To do so we should place the patient in a suitable position. In default of a proper couch specially made for the purpose, I place him reclining in an easy arm-chair, with the buttocks near the edge of the seat, and with the thighs well separated. Kneeling between the latter I introduce the tube with plug well oiled, until, by its direction,

I know that it has passed the triangular ligament of the perineum. I then introduce the index finger of the left hand (previously well greased) into the rectum, and guide the passage of the tube through the membranous portion of the urethra into the prostatic region. So soon as it has traversed the latter, I withdraw the finger from the rectum, extract the plug from the tube, and attach the endoscope, which should previously be lighted and adjusted. Now, holding the endoscope in the left hand, I proceed gradually to withdraw it at the same time that I keep my eye closely applied to the eye-piece of the instrument.

As the tube passes each portion of the canal, the lining membrane comes into full view bit by bit. If a difficulty occur in seeing any portion it will generally be found to arise from oil, blood, mucus, or some other fluid, obscuring the surface. This difficulty is easily remedied.

Before proceeding to the examination a number of rods, with screw ends, should be prepared by winding the extremities with cotton wadding, and some should be provided with portions of fine moist sponge securely tied on.

When any obscurity of vision arises, one of the rods should be taken in the right hand, introduced into the central slit in the tube, and passed down to its extremity; a little pressure and a rotation or two suffice usually to take up the fluid, and the rod may then be withdrawn. Sometimes it will happen that the wadding or sponge is left behind, and to meet this emergency a little blunt hook should be at hand; this may be passed down to the sponge or wadding and rotated once or twice; though blunt it immediately becomes engaged therein, and by its aid the substance can be withdrawn. I prefer this hook to Desormeax's miniature corkscrew, as being much safer and less liable to do mischief. During an endoscopic examination this wiping of the part may frequently need repetition. The screw end on the rods facilitates the removal of the cotton when soiled. It is hardly necessary to observe that the strictest care is required in the cleansing of the endoscope and its appurtenances, lest it ever might become a medium for the transmission of infection from case to case.

In the introduction of the endoscope catheter it is necessary to be careful not to enter the bladder lest a rush of urine fill it up and embarrass the examination. This is no needless precaution, for habit will teach that a straight instrument enters the bladder with almost as much facility as a curved one. In order thoroughly to investigate the canal it is well, while withdrawing the endoscope, occasionally, to reintroduce it a line or two, and to increase or diminish the light; thus views are obtained which may clear up points otherwise somewhat obscure.

I may also observe that the index finger of the right hand, well oiled, may, from time to time, be slipped into the rectum during the examination of the deeper portions of the urethra, as by its aid we are able to ascertain the position of the extremity of the tube, and to identify the portion which may be diseased. Thus I have frequently been able to ascertain, with positive certainty, morbid conditions of the prostatic, membranous, and bulbar sections of the canal. In cases of stricture or disease anterior to the bulb is not requisite to place the patient reclining on a couch—he may simply stand with his back against the wall, and the operator kneeling in front of him. I should here mention that the optical portion of my endoscope has been made for me by Messrs. Spencer of Aungier-street, Dublin, while the specula and other fittings were made partly by Charrière of Paris, and partly by Messrs. Thompson and O'Neill of this city. Messrs. Fannin have given me to understand that they intend to supply the instrument perfect in all details.

[While all other varieties of urethritis tend to a natural cure, the result of specific contagion frequently merges into a chronic form characterised by a slight colourless discharge, generally most noticeable in the morning. By the endoscope the mucous membrane from the bulbar to the prostatic portion is seen to be granular, exactly like the conjunctiva of a chronically inflamed eyelid.]

This once ascertained, treatment is a matter of facility. Cauterization which cures granular conditions of mucous membranes elsewhere—in the eyelids, uterus, &c.—will cure it here; and, thanks to the endoscope, can easily be applied to the exact spot diseased. Some caution is needed in the application of caustic for the cure of granular urethritis. The method which I prefer is as follows:—Having brought the anterior portion of the granulating surface into view I first carefully wipe it dry with a portion of wadding carried down by a suitable rod. I next take a second rod, armed also with cotton wadding, and, having dipped it in a solution of nitrate of silver or sulphate of copper (xv.—xxx. gr.—℥i), pass it down to the diseased part and hold it in contact therewith for a few seconds. I then withdraw the rod and endoscope catheter. This operation causes very bearable pain, which may be removed by a warm bath and moderate anodyne. In the milder cases when next we proceed to use the caustic—perhaps in the course of four or five days—we find that the portion last touched is healed and that the endoscope catheter penetrates further than before without causing pain. A second portion may now be cauterized, and in like manner, by degrees, the whole diseased surface cured.

Under the influence of the caustic the granulations disappear—certainly, though rarely quickly—submucous effusion is absorbed—constriction disappears—and finally, the parts return to a condition of health from which they are not liable to relapse spontaneously.

[Much may also be learnt regarding the nature of strictures. The orifice of the stricture can in most cases be easily made out.]

When the stricture is brought fairly into view, and its orifice discovered by means of the probe, the operator can see the exact disposition of the indurated parts, and can easily, under his sight, divide the stricture, confining his incisions to the indurated and callous parts. M. Desormeaux uses for the purpose a miniature button bistoury, with a handle. I would venture to propose instead a blade somewhat different, which has a long probe point, and a short cutting edge, which is blunted towards its posterior extremity, so as to limit the incisions to the contracted parts, as in Maisonneuve's urethrotome. Such precaution is hardly necessary, as the stricture can be incised under the eye of the operator, but it is an additional safeguard against an over extensive division of the parts. The stricture being in view, and the orifice discovered, this knife should be introduced carefully into the catheter of the endoscope, so as to avoid injury to its cutting edge by contact with the interior of the tube. The catheter may then be rotated, so as to bring its slit into the requisite position to enable the operator to turn the edge of the urethrotome in any direction he may think desirable. Now its probe point may be engaged in the orifice of the stricture, and the instrument gently pressed onwards until the constricted parts yield. The after treatment consists in the use of quinine and opium to prevent urinary fever, and careful dilatation to maintain the incision patent.—*Dublin Quarterly Journal, May* 1865, *p.* 329.

DISEASES OF THE SKIN.

61.—ON THE NATURE, THE VARIETIES, AND THE TREATMENT OF ECZEMA.

By Erasmus Wilson, Esq., F.R.S., (From a paper read before the British Medical Association, at Cambridge, Aug., 1864.)

The prevalence of eczema is so considerable, that its name has taken root among the population, even as a "household word." There is scarcely a medical practitioner in Great Britain who has not, at this moment, a case of eczema under treatment; or who, within a limited period of time, has not had such a case brought

within his observation. Eczema is common to all classes of society; is restricted neither by age nor sex; is equally frequent in the male and in the female; and is met with in the infant of a few weeks old, as commonly as in those who have reached the more protracted term of human existence. In one thousand cases of cutaneous disease of every denomination, there occur 298 instances of eczema; very nearly one in every thirty, or, in other words, thirty per cent.

What is eczema? Eczema is an inflammation of the skin, attended with alteration of its structure, and derangement of its functions. It is more vascular than in health, its vessels being in a state of congestion; its sensibility is morbidly increased, sometimes taking on the character of itching, tingling, or smarting, and sometimes that of pain; it is thickened by infiltration of serum into its tissues; is sometimes fissured and sometimes œdematous; it exudes a serous lymph at various times and in various quantity, sometimes in excessive abundance; the epidermis is sometimes raised into vesicles, sometimes wholly removed; and is reproduced unhealthily, so as to form mucopurulent secretions and squamæ of various size; and sometimes is replaced by a crust of greater or less thickness, resulting from the desiccation of the morbid secretions.

But eczema rarely presents all these characteristic symptoms in an equal degree; and the diversity of the symptoms; and the modification which those symptoms undergo in different parts of the skin, in different constitutions, and in different temperaments, are the foundation of its varieties. As the varieties of eczema are founded on the prominence of one or other of the symptoms of the disease, they may be easily designated and as easily recognised. For example: when the prevailing sign is redness, the eruption is an eczema erythematosum; when the pores of the skin are erected into the form of papulæ, the disease is an eczema papulosum; where there are vesicles, the variety is eczema vesiculosum; where excessive exudation of a serous or colourless lymph is the leading sign, the case is one of eczema ichorosum; where there are pustules mingled with the other signs, it an eczema pustulosum; and, where, as in the chronic forms of the affection, the epidermis is scabrous and scaly, the disease is an eczema squamosum. These terms comprehend the principal forms of eczema dependent on symptoms; but additional terms suggest themselves naturally for different conditions; for example: the skin is sometimes cracked and fissured —eczema fissum. Sometimes the thickening and condensation of the diseased tissues, in a chronic stage of the affection suggest the terms eczema sclerosum and eczema verrucosum. Sometimes the state of the part is aptly expressed by the term eczema œdematosum; sometimes, from the nature of the secre-

tion, eczema mucosum. And, more than once, we have met with a case to which, on account of the severe pain by which it was accompanied, the term eczema neurosum was correctly applicable.

It is rare to find an eczema perfectly simple in its eruption, and capable of being represented singly by any one of the terms already mentioned. More frequently, two, or indeed several of the forms may be present at the same time, and sometimes, even the whole. For example: in one case we may have an erythematous eczema combined with papulæ, which we should then distinguish by the name of eczema erythematosum et papulosum (eczema lichenodes); or, it might be an eczema erythematosum et ichorosum; or, if the exudation of fluid from the inflamed surface were the most striking feature, we should name it an eczema ichorosum, which might be, but in a less degree, erythematosum et papulosum: or even pustulosum. Or, the case might be one of eczema vesiculosum et erythematosum; or, an eczema squamosum (eczema chronicum), which is, at the same time, in part, fissum; and in part, ichorosum. These terms have the advantage of being simple, expressive, and clear; they rise to the mind as freely as the terms: red, pimply, oozy. mattery, or scaly; and, they render the narrator intelligible to every one conversant with medicine and medical language; while the commoner appellations eczema simplex and eczema rubrum, convey to the uninstructed in cutaneous pathology, either an erroneous idea or absolutely nothing at all.

Varieties of eczema are also derived from situation; from the part of the body attacked with the eruption. For example, we have eczema capitis; eczema faciei; eczema aurium; eczema palpebrarum; eczema oris et labiorum; eczema axillarum; eczema mamillarum; eczema umbilicale; eczema inguinum; eczema pudendi; eczema perinæi; eczema ani; eczema articulorum; eczema manuum et pedum; eczema dorsi manûs; eczema palmare et plantare; eczema digitorum; eczema inguinum, &c. Moreover, we are taught by experience that in certain situations we shall be most likely to meet with one or other of the established varieties of the disease. For example: eczema capitis et aurium are commonly ichorosum in a recent attack, and squamosum at a later period; eczema articulorum, eczema axillarum, eczema inguinum, and eczema pudendi, are also very frequently ichorosum, and sometimes mucosum. Eczema of the fleshy parts of the forearms and legs is not unfrequently vesiculosum; as is also eczema digitorum. Eczema dorsi manûs is usually papulosum; while eczema palmare is squamosum et sæpe fissum.

The extent of the eruption is also to be considered in designating its varieties. It may be general or it may be partial.

When partial, the patches of the disease may be single or multiple; they may be defined (figuratum); or diffused (diffusum). Sometimes they suggest the idea of the dimensions and figure of a piece of money, nummulare; and sometimes they are bounded by a prominent ridge (marginatum) and spread by the circumference, while the inflammation subsides within the included area; this is especially the case in a curious form of eczema marginatum common in India, where it is known as the Burmese ringworm.

Willan and Bateman include all the varieties of eczema under the three heads; solare, impetiginodes, and rubrum, corresponding with the eczema vesiculosum, eczema pustulosum, and eczema ichorosum of our improved nomenclature. Hardy proposes four varieties; namely, simplex, rubrum, fissum (fendillé), and impetigo. Hebra employs the suggestive terms squamosum, papulosum (lichenodes), vesiculare (solare), madidans (rubrum), and impetiginosum.

It will be perceived that Hebra makes eczema squamosum the first of his forms of eczema; whereas I take it to be the last. Let me explain. Eczema erythematosum is commonly accompanied with desquamation; and when the eruption is general, this is remarkably the case. Now, Hebra selects the moment when it is coated over with scales as the type of this form of the disease. But, it must be remembered that the scales may be disposed of in a variety of ways; there was a time, before which they were formed; there is a time when they have ceased to be produced; there is an intermediate time when they may be removed artificially by dressings, poultices, or baths. What are we to call the disease when the scales and desquamation are no longer present? The answer is plain; the redness, the erythema remains; indeed, the erythema is always there; from the beginning before the scales existed, at the end, after they exist no more; hence, the term "erythematosum" appears to me to be more appropriate than "squamosum." Again, there are forms of eczema in which there is redness, but nothing deserving to be called scales; and to these cases, which are numerous, the term "erythematosum" is especially applicable.

On the other hand, when eczema reaches its latest period, when it is chronic, there is still redness; but scaliness and desquamation are a more predominant character than redness; and it is to this period, that I prefer to attach the term "squamosum." Eczema squamosum, I regard as the chronic stage of eczema; whilst eczema erythematosum, which may also be chronic, applies with especial aptitude to the earliest stage of the eruption. Eczema squamosum is, therefore, in my view of the pathological indications of the disease, an eczema chronicum squamosum, and is the true representative of the much abused

term psoriasis. Psora represents the more active, and especially the exudative forms of eczema; and psoriasis, its chronic, its dry, and its desquamating form.

The Romans, who designated eczema by the name of scabies, drew attention to a characteristic distinction between its moist and its dry form—scabies humida and scabies sicca. The Greeks appear to have had the same object in view in employing the words psora and psoriasis; the former term corresponding with scabies humida, the latter with scabies sicca. The distinction is a radical one, and of the first importance in the study of eczema. Eczema siccum, which is represented by eczema erythematosum, eczema papulosum, and eczema squamosum, is specially distinguished by an absence of exudation, or by its presence in a very slight degree; whereas eczema humidum, represented by eczema vesiculosum, eczema ichorosum, and eczema pustulosum, is remarkable for the exudation of a serous and sometimes a muco-purulent fluid, often in astonishing quantity —a positive flux. According to our observation, the dry forms of eczema more than double in frequency those of the moist or humid kind; therefore a classification that assumes for eczema a definition dependent on exudation as a pathognomonic sign, must, of a necessity, be incorrect.

Treatment of Eczema.—If the foregone premises with regard to the causes of eczema be correct, the treatment of eczema resolves itself into a treatment of debility—a treatment of constitutional debility, and a treatment of local debility—and the influence which we are called upon to exercise over our patients is one which shall have for its object to restore power, and thereby to regulate function and to confirm health; for health is nothing more than correct function united with normal power.

In general terms, our constitutional treatment must be directed to the regulation of the functions; and, concurrently with regulation of functions, to the restoration of the tone, the vigour, the vitality of the general system. In the most numerous group of cases, those which depend upon debility of assimilation, our attention is principally directed to the digestive organs and the secretions. In the next most numerous group, those which are due to nutritive debility, the digestive and secreting organs must also be carefully examined, although they are less likely to be faulty than in the preceding group; and we are enabled sooner than in the former case to resort to our strengthening remedies. The same remarks apply to the third group, originating in nervous debility; while, in these latter, we have to deal with the special indication of weakened power of the nervous system.

If we turn our attention to the tonics, which may be found suitable for the treatment of eczema, we find: simple vegetable

bitters, cinchona and quinine, chalybeates, the mineral acids—hydrochloric, nitric, sulphuric, and phosphoric; and above all, arsenic. Among simple vegetable bitters, with what respect must we speak of gentian, calumba, orange-peel, quassia, chirayta, chamomile, and hop; in infusion or decoction; in extract or in tincture; in combination with the mineral acids, or with ammonia, soda, or potash. How admirably cinchona and quinine perform their part; cinchona in decoction or tincture, or liquor, with sulphuric acid or with ammonia; and quinine in solution, with an excess of sulphuric acid. How perfect a remedy is one grain of quinine, with one drachm of sulphate of magnesia, seven minims of dilute nitric acid, and an ounce and a half of infusion of roses, both as a gentle aperient, a corrector of acidity, a promoter of mucous secretion, and as a tonic. Then the chalybeates, iron and its salts, the sulphate, the tincture of the hydrochlorate, the citrate, the superphosphate, the iodide, and the citrate of iron and quinine. Next, the mineral acids combined with the bitter tonics, and especially the nitro-muriatic and the phosphoric. Again, as a nutritive tonic, we have the cod-liver oil; and our list of remote predisposing causes will discover several sections in which that remedy is peculiarly applicable; for example: strumous diathesis, excessive growth, and sexual excess.

All that precedes applies to any disease of the human frame; whether it be of the lungs, the heart, the liver, the brain, the kidneys, or the womb; the treatment now under consideration is catholic and not special; its aim, to bring back normal function, and so to restore health; it is the routine of the physician's art; but there is a remedy which possesses a special influence on the skin, and has long been held sacred in cutaneous medicine, namely, arsenic. When all has been effected that can be accomplished by aperients, alteratives, derivatives, and tonics, then comes the reign of arsenic; and at that moment, arsenic is triumphant. But arsenic must be regarded, not as our common weapon, but as our reserve; to be administered when it can do no harm, and when there is every probability of its doing good. Arsenic is a tonic, a tonic which influences chiefly the nerve-substance, and not only gives force to, but improves the nutrition of the nervous matter; in this way it operates upon the trophic system of nerves throughout the economy; and in cutaneous medicine we have evidence of its remarkable powers upon the skin. Arsenic accelerates those actions of the skin which tend to its nutrition and renovation, and therein it becomes a healer of its eruptions, and a valuable aid to the cure of eczema. But arsenic, above all medicines, must be handled with judgment and care; it must be jealously watched, lest it give rise to any unfavourable symptoms, and its use must be

suspended at once if there exists even a doubt of its healthful operation. Above all, it is to be borne in mind that only at the right moment is arsenic applicable, and that right moment can only be determined by watchful care, or by long experience.

Our *Pharmacopœia* is rich in preparations of arsenic; but the most valuable are, the liquor potassæ arsenitis, and the liquor arsenici chloridi; the dose of the former applicable to an adult suffering with eczema, is two to four minims; and of the latter, double that quantity. Some years ago, in a paper on Eczema Infantile I laid before the British Medical Association a formula for the administration of Fowler's solution; and experience has assured me that no better form can be employed for its administration.* Arsenic should always be administered in small bulk, and the formula in question gives one drachm of fluid as the dose, and always on a full stomach, or, if possible, in the midst of a meal. The object of these precautions is obvious; arsenic is intended especially as an improver of nutrition, and therefore should be made to enter the stream of blood with the chyle; in its nature it is an irritant to the mucous membrane, and, therefore, it should be administered at a time when the lining membrane of the stomach is coated with mucous secretion, and at the same time when, by mixing with the mass of the meal, the remedy is largely diluted.

All the processes of renovation which depend upon nutrition are slow; hence, as we look for the good effects of arsenic in the improvement of nutrition, we must be prepared to continue its use for a considerable time. This is another reason for administering arsenic in small doses, independently of the suggestions to the same effect gathered from experience. Indeed, by employing it in larger doses, we naturally frustrate the objects which we ourselves have in view; we set up the irritant action of the medicine; we excite nausea, sickness, pain in the stomach, or alimentary canal, prostration of nervous power, or cough, and then we are compelled to suspend it. While, on the other hand, administered with judgment and discretion, there is no safer medicine in the *Pharmacopœia*; and, at the same time, in my opinion, there is none to equal it in excellence and usefulness.

To be a successful practitioner in the treatment of eczema, a medical man must be an accomplished physician; to manage the local treatment with success, he must also be an able surgeon. The local treatment of eczema must be conducted according to the general principles of surgery. The inflamed part must be soothed in the acute stages of the disease, it must be supported

* ℞. Vini ferri ℥iss.; syrupi simplicis ʒiij.; liquoris potassæ arsenitis ʒj.; aquæ puræ ℥ij. Misce. Fiat mistura; ʒj. pro dosi.

and stimulated during the chronic stages, and it must be excited to a new action in the most chronic stage of all. To soothe, we must employ water-dressing, unguents, cerates, well adjusted bandages, and rest. To give local tone, we must have recourse to stimulant applications of various kinds.

The water-dressing is useful where there is heat, pain, itching, or tension, but should not be continued beyond the period during which those symptoms exist; for, when prolonged for too great a time, it lowers the tone of the tissues and perpetuates the eruption. In our list of remote predisposing causes, it will be seen that one amongst them is "moisture with heat," the exact condition which at one moment of the disease we employ with advantage in the use of the water-dressing. Whereas, we could hardly adopt any more certain means of producing eczema artificially, than by the application for a long time of water-dressing to a healthy part of the skin. This operation is often seen in practice, in the eruptions which follow the lengthened use of poultices.

When the local inflammation partakes of the sub-acute character rather than the acute, and when the water-dressing has effected the purpose for which it was applied, we may have recourse to cerates or ointments. As a preliminary to both of these appliances, all crusts and sordes should be carefully wiped or washed away from the inflamed part; if they be dry and hard, the water-dressing or the ointment-dressing may be applied with a view to soften them, and then they should be removed, and the dressing with the ointment nicely adjusted, and, wherever practicable, secured in position by a bandage: where this cannot be done, then we must have recourse to adhesive straps. In selecting an ointment, the best with which I am acquainted is the benzoated ointment of oxide of zinc. It should be spread thickly on the lint; the dressings should be shaped so as not to overreach the sound skin; and the roller of elastic cotton bandage should be put on smoothly, and with a view to produce equable pressure on the eruption, and support to the vessels of the limb. On a part of any extent it is more convenient, and we avoid wrinkles thereby, to apply the dressing in slips, and dispose them in the manner of a many-tailed bandage; and when a limb affected with eczema is carefully packed up in the manner described, it may be left for twenty-four hours, and in more chronic cases for forty-eight hours, without disturbance; then it should be unpacked, re-dressed, wiped with a soft napkin to remove sordes, and packed up again for a similar period. While packed up in this way, the itching of an eczema commonly ceases, and the patient is saved from the annoyance and suffering of that disagreeable symptom, while the skin returns gradually to its healthy condition. If there

should be heat and burning of the limb when packed, the bandage may be sopped with water, and then we get the advantage of the water-dressing superadded to the tonic treatment.

Where great irritability prevails in the constitution of the patient, we require to have recourse to sedatives; and where irritability is present in the part, we may find it difficult to contrive a dressing which will perfectly suit the eruption. Sometimes grease of all kinds acts as an irritant, and then we are driven for a while to the use of lotions or powders. Sometimes a sedative, such as the acetas plumbi, added to the ointment, relieves the irritability; sometimes a more stimulating application, such as the unguentum resinæ flavæ, or the ointment of juniper tar. At other times, we may put an end to the irritability by pencilling the surface with a weak solution of the nitrate of silver in nitric ether. Again, we rarely fail to relieve the irritability by undressing the part, washing it thoroughly with the juniper tar soap, drying it, and dressing and packing it up again as before. The successful treatment of eczema needs many resources; but only such as may be explained on the recognised principles of physiology and surgery.

The erythematous form of eczema yields very kindly to the benzoated ointment of oxide of zinc: so also do the moist forms of the eruption, the dressing bring aided by moderate compression with a bandage. Eczema ichorosum must be followed up patiently with this plan of local treatment, until the ichorous secretion ceases to be formed, and the eruption heals. But the chronic forms of the disease represented by eczema squamosum, require a stimulant treatment. Washing with the juniper tar soap, dressing with the unguentum resinæ flavæ, pencilling with a solution of nitrate of silver, or a strong lixivium of potash. Whenever these stronger local remedies are used, the eruption should be subsequently dressed, *more dicto*, with the benzoated ointment of oxide of zinc, and then carefully packed up.

In the moist forms of eczema, there is always a considerable quantity of serous lymph present in the tissues of the skin; and, until that serous fluid is removed, either by absorption from within or by excretion from the part, the skin cannot return to its normal state. It is to the presence of this fluid infiltrated in the substance of the skin, that the chronic forms of eczema owe their thickening, their induration, and their rupture; the latter giving occasion to fissures and cracks, and being most conspicuous where the skin is naturally thick and wrinkled, as on the hands and fingers, or the feet, behind the ears, along the borders of the mouth, and in the cleft of the podex. For this state of the skin, in an aggravated form, there is no remedy to compare with a solution of caustic potash. That applica-

tion seems to purge the skin of its abnormal fluids, to promote absorption, and to give energy to nutrition; and it is amazing with what rapidity the cracks will heal, and the most chronic state of disease get well under this treatment.—*British Medical Journal, Nov.* 19, 1864, *p.* 567.

62.—ON THE TREATMENT OF ECZEMA.

By R. H. Meade, Esq., Senior Surgeon to the Bradford Infirmary.

[Mr. Meade quite agrees with Erasmus Wilson that eczema is generally a disease of debility. Mr. Wilson's remedy *par excellence* is arsenic. Mr. Meade more frequently uses the bichloride of mercury and antimony, and his reason for this is, that though it mostly arises in weak states of constitution, it is in its own essence inflammatory, and therefore]

While the strength is supported by generous diet, an alterative remedy, like bichloride of mercury, which is so generally efficacious in chronic inflammatory complaints, may be advantageously given, and even combined with tonics.

With regard to antimony, it is less generally applicable in eczema than corrosive sublimate, but it will sometimes be found very useful; and in some obstinate cases, I have seen these two remedies exceedingly efficacious in combination. The form of the disease in which antimony administered singly seems more particularly applicable, is that in which it is acute in its character, and occurs in persons of a full inflammatory habit, who are also, perhaps, subject to gout or rheumatism. Tartar emetic given here, in combination with aperient salines and magnesia, will often cure the complaint very quickly.

We frequently see very annoying cases of eczema in oldish people, in whom the skin round the anus and about the scrotum or labia is affected. I have found the combination of bichloride of mercury and tartar emetic in the same mixture particularly useful in these cases, and sometimes cure them in a week or two, when they had resisted other treatment for two or three years. The dose of bichloride which I generally find sufficient is one-sixteenth of a grain, given three times a day, with one-eighth of a grain antimony; in some demulcent mixture, as decoction of sarsaparilla or dulcamara.

Eczema very frequently occurs in infants, breaking out soon after birth, and becoming aggravated as teething commences. Most of these cases have, I believe, an hereditary origin—one or the other parent having a disposition to the complaint. These young subjects are mostly weak, and require strengthening treatment. Unless they are suckled by a strong mother or wet-nurse, they should have good beef-tea or broth given them, in

addition to the milk; and have little or no farinaceous food. These are cases which will be especially benefited by corrosive sublimate. It must be given in very small doses, and continued a long time; it may be taken with occasional intermissions for months together, with advantage. A sixtieth of a grain, or even less, in a very young child, taken twice a day, with a little fluid extract of sarsaparilla and glycerine (which is a very good demulcent), will mostly check the complaint. Should the bowels be at all irritated by the mercury, a little opium may be combined with it.

In recommending bichloride of mercury so strongly as a remedy for eczema, I do not wish it to be inferred that I am bringing forward anything new; for I believe that this medicine has been long and largely used in this complaint, by the medical officers of the London Hospital for Diseases of the Skin. Having long used it in my own practice, I am anxious to bear testimony to its efficacy; and can recommend those of my medical brethren who are not yet acquainted with its virtues in the treatment of eczema, to give it a fair trial, both in combination with antimony and without it.

In the few remarks which I have made upon the treatment of eczema, I have made no allusion to external applications. I now mention them, for the purpose of saying that I by no means underrate their importance; but, as I am not professing to write a paper on the general treatment of this complaint, but only wishing to call attention to one or two particular remedies, I think it unnecessary to enter into the subject.—*British Med. Journal, Dec.* 17, 1864, *p.* 673.

63.—ON ECZEMA OF THE EYELIDS, CONJUNCTIVA, AND CORNEA.

By Furneaux Jordan, Esq., Surgeon to the Queen's Hospital, Birmingham.

Many observers, and especially writers on diseases of the skin, have considered ophthalmia tarsi to be simply eczema of the lids. Dr. Mackenzie has pointed out that scrofulous, or as he terms it, phlyctenular ophthalmia, is frequently accociated with eruptions on the skin. It is the object of this paper to show that not only ophthalmia tarsi is eczema of the lids, but that granular lids, a peculiar swelling of the sub-integumental connective tissue of the lids, lipptudo, strumous ophthalmia, certain forms of simple or catarrhal ophthalmia, keratitis and strumous keratitis, and certain ulcers on the cornea, are merely varieties of eczematous disease. Cases of extreme, firm, indolent, pale or pinkish swelling of the lids, occur occasionally, the only cause of which is eczema of the margins of the lids. The

eczema may be very slight, or it may pass away quickly, and leave only the swelling behind. Unchecked eczema of the eyelids terminates in lippitudo just as persistent and progressive eczema of the cornea produces pannus. Both these conditions are analogous to the eczematously red, swollen, and moist condition of the skin which may persist for an indefinite period. Eczema of the conjunctiva presents many important features. The so-called strumous ophthalmia may be regarded as chronic eczema. The several stages of pimple, vesicle, ulcer, or thickened patch, admit of indisputable demonstration. In acute ezema of the conjunctiva, there is for a few days a uniform scarlet-colour; then a crowd of vesicles, which soon pass away, and leave an irregular or patchy redness—each patch, however ll-defined, having a redder, thicker, and possibly ulcerated centre. These cases have a slight muco-purulent discharge, and are always tedious. If treated as eczema, they speedily recover. The so-called keratitis, or strumous keratitis, is eczema of the cornea. When vesicles, white patches (necessarily white because of the anatomical structure of the cornea), or ulcers occur on the cornea in conjunction with vesicles on the conjunctiva, the term scrofulous ophthalmia is commonly used. If the same pimples (necessarily flat), vesicles, patches, or ulcers occur on the cornea alone, especially near its centre, the term keratitis is applied, notwithstanding the symptoms are similar, and notwithstanding that there is usually, if it be carefully sought for, evidence of eczema of the lids or face, or ears or scalp. The characters of eczema of the cornea are quite as typical as they are of eczema elsewhere. The several varieties of eczema of the cornea, conjunctiva, and lids are combined in a great variety of modes. They are much more frequently combined than not, and very frequently indeed associated with cutaneous eczema in its favourite localities. Eczema is often limited to sites as small as the cornea. The treatment should be directed to eczema. Its chief features are non-stimulating diet and alkaline medicines, with a little iron added in most cases. If the lids are affected, as also in pannus, lippitudo, and granular lids, a little of any of the "eczema ointments" may be used, with the customary attention to details; if much photophobia, a little morphia may be given in the morning.—*Lancet, Jan.* 21, 1865, *p.* 62.

64.—CASE OF SYCOSIS MENTI VEL MENTAGRA.

Under the care of Professor Laycock, at the Edinburgh Royal Infirmary.—(Reported by Dr. Dyce Duckworth, Resident Physician.)

[The patient was twenty-one years of age, and stated that two

years before admission a scale formed under the left angle of the mouth, in consequence of cold, and this gradually spread.]

On admission, the upper lip and chin were seen to be covered with a yellow scab matting the hair of his moustache and shortly-clipped beard. In parts small isolated pustules were seen, turgid with pus and having a hair issuing from their summits. On closer examination, it was obvious that the parts exclusively affected were those from which the beard and moustache grew; thus the eruption occurred at the angles of the mouth and at the parts below the centre of the lower lip—the site of the "imperial," but not in the whiskers. No pain was complained of, and there was no itchiness.

He was ordered to foment the chin well with warm water, and then to apply a strong alkaline lotion consisting of an ounce of the bicarbonate of potass to four of distilled water. This was with the view of dissolving the crusts. The hair being closely clipped to the level of the skin (for this is always necessary in using any lotion to the hairy surfaces), the lotion was applied on lint by means of a mask of oiled silk, which passed under the nose, included the entire chin, and was fastend behind the head. The parts were fomented with warm water twice daily. Dr. Laycock diagnosed the affection as sycosis. The fig-like character of the disease was more especially manifested on the 29th October, when the crusts and scabs being removed the pustular points were seen standing out on a darkened red base, presenting very much the appearance of an opened fig. The alkaline lotion was continued for a few days more. Dr. Laycock directed attention to the following points in connection with the case. He stated that this affection is considered to be of parasitic origin and infectious, and cited cases which had occurred in his own experience which proved the truth of this belief.

"If this were so, then why did the disease not attack this man's whiskers?" The disease was proved to be communicable from chin to chin, but the whiskers escaped this particular form. He stated that there was a form of sycosis which attacked the whiskers solely, and not the skin. According to the Professor, there are three distinct portions of the facial hair, and the development of each is found to differ in different men. There is first the jaw hair or masseter beard; some men have this alone and no other kind. Next there is the chin hair, with that of the upper lip—the moustache—constituting the muzzle hair or beard. This is first developed, and very commonly the only part of the facial hair that is developed. And lastly there is the face beard, or that between the jaw and the muzzle beards, over the zygomatic and buccinator muscles. The face

beard most commonly accompanies the jaw beard. The muzzle hair is in relation with both the ovaria and testes, and is apt to be fully developed in women sometimes, as in the *Fœminæ barbatæ*.

It appears that there are three forms of sycosis which may attack different portions of the beard.

Dr. Laycock pointed out how skin affections generally, and those affecting the hair regions in particular, follow this law. Even pediculi have their proper hairy habitat. He believes that there are special centres of innervation and nutrition for each of the before-mentioned portions of the beard, and are indicated not only by the varying development but also by the differences of each as to earlier and later greyness; and further, that there are definite peripheral nervous distributions in these parts which probably the scalpel will never reach. It is this consideration, then, which points to the pathology of these somewhat anomalous cases. We must believe that there are certain regions rendered liable to the morbid invasion by defective nutrition, while others, perhaps in immediate proximity, though in another nervous area, remain free from and unaffected by the existing morbific influences, because their nutrition is healthy. Now, in this case the disease never spread from the muzzle hair.

The alkaline lotion was discontinued, and it was decided to treat the case with arsenic locally. There was ordered, ℞. Glycerinæ, f. ℥x.; liquoris potassæ arsenitis, f. ℥ij. M. ut fiat lotio. To be brushed into the affected parts night and morning after ablution.

Two days after this treatment had been practised, the chin looked red and irritated. The arsenical glycerine was discontinued, and the alkaline lotion ordered to be reapplied.

November 7. The parts are improved in appearance. The arsenicated glycerine is being employed again.

13th. It seems that the disease has been spreading a little below the chin, and the pustules lie on an irritable red base. General health good. To discontinue the arsenicated glycerine lotion, and apply nothing to the parts. Dr. Laycock showed how the arsenical preparation had acted in inducing an active inflammatory condition: this was a step towards the removal of the disease. It is a peculiarity of arsenic that the inflammation it excites entails a new and modified action of the part subsequently. Sufficient inflammatory action was induced in this instance, and it was now prudent to suspend the remedy, and wait for the ultimate results of it.

19th. The pustules have dried up and formed crusts in various portions of the chin. The skin below is much less red. Ordered to wash the chin several times a-day with olive oil and

the yolk of an egg mixed, instead of soap, and to foment the parts with warm water subsequently. The hairs to be clipped short with sharp scissors. The patient was decidedly better from this time, and but few pustules were seen to be forming. They were most numerous in the centre of the chin over the site of the imperial beard.

In two days after this all the scabs were removed, and a thickened, discoloured base was to be seen where they formerly existed.

30th. The chin and upper lip are now nearly clean. Daily ablution with oil and yolk of egg was practised, and the hair kept short by clipping. A few pustules still to be seen in the centre of the chin, and one or two below it. These were ordered to be punctured with a needle and let out, afterwards to be touched with the arsenicated glycerine.

December 5th. The above treatment has been practised, and the same process has occurred which was before described, viz., the formation of a crust, and resolution of the inflammatory action. The parts to be washed twice daily with oil and yolk of egg and fomented.

13th. All the pustules have disappeared, and the skin is merely reddened. Discharged this day.—*Medical Times and Gazette, Dec.* 17, 1864, *p.* 650.

65.—ON THE ACTION OF CREASOTE IN THE TREATMENT OF SYCOSIS.

By Dr. Masse.

Dr. Masse in a note on this subject, says:—"The patient whom I was authorized to treat, in the large Hôtel-Dieu at Montpellier, was a young soldier who had a pustulo-tubercular eruption upon the cheeks and chin; he exhibited, moreover, *herpes circinatus* upon the back of his hand. He had contracted this malady while being shaved by a barber whose razor or linen, plentifully supplied with the parasites, had thus communicated the same affection to five or six other soldiers. I coupled microscopical examination with the information I had acquired as to the etiology of this affection, with a view of completely investigating the nature of it. I discovered, in the hypertrophied hair-bulbs, spores and tubes of the *mycelium* of a veritable *Microsporon mentagrophyte*. The treatment employed consisted of lotions, composed of 50 grammes of water, 50 grammes of alcohol, and 50 centigrammes of creasote, and which were used twice a day for a week. At the end of this period the amount of creasote in the lotion was doubled, the latter being applied three times a day. By the end of another week the pustulo-

tubercular eruption had completely vanished. Microscopical examination failed to exhibit any parasites, and the invalid had completely recovered. This novel method which I came to make trial of cannot be said to be empirical treatment, since it is but a medical adaptation of the recent investigations of M. Béchamp upon the action of creasote in fermentation, and according to which creasote was found under these circumstances to check the development of the spores of the vegetable cryptogams, as well as of the ovules of the infusoria. The cryptogamous parasites appearing to me, viewed with regard to their organization, to differ but little from the mould developed during fermentation, I thought that creasote might in a like manner arrest the formation of parasite spores in diseases of the skin. By destroying the source of a malady we should effect a cure. Creasote does not immediately destroy the cryptogam of which the mycelium is already developed, but it renders the spores incapable of germinating; after a short time the mycelium becomes dried up, and the parasite dispppears."—*Gaz. des Hopitaux; and Glasgow Med. Journal, Jan.* 1865, *p.* 482.

66.—ON THE TREATMENT OF SCABIES.

By Dr. J. F. Nicholls, Surgeon to the Royal Wilts Militia.

[Dr. Nicholls has tried the following plan in several hundred cases, and where the remedy has been properly prepared and used it invariably effects a cure.]

The contrast is extremely great between this and the old plan of treatment, as was shown by the men of my own regiment being only kept in hospital two or three hours, and then sent to their duty, while in the same hospital, men of other regiments treated according to the old method, have been kept for eight and ten days, nearly naked, with only a blanket or two saturated with sulphur ointment to cover them, and in an atmosphere of the most awfully offensive character.

The remedy which I have adopted is prepared by boiling one part of quick lime, with two parts of sublimed sulphur, in ten parts of water, until the lime and sulphur are perfectly united; during the boiling it must be constantly stirred with a piece of wood, and when the sulphur and lime have combined, the fluid is to be decanted and kept in a well-stoppered bottle.

The way in which I have it applied is as follows:—Every part of the body, with the exception of the face and scalp, is well rubbed with the preparation for half an hour, and then washed with soap and hot water. After the application an entire change of clean linen and clothing is put on. The

patient's linen is disinfected by being soaked in boiling water, and his clothes are freed from contagion by being subjected to a temperature of about 200° Fahr., in an oven, for a short time. When using the remedy for young children I have had it diluted, by mixing one part of the solution, prepared as above, with three parts of water.—*Medical Mirror, March* 1865, *p.* 169.

67.—A NEW AND SPEEDY METHOD FOR THE CURE OF SCABIES.

Dr. Decaisne, physician to the garrison at Antwerp, has addressed a communication to the Belgian Academy of Medicine regarding an almost instantaneous mode of cure of scabies in man. He proposes, says the *Presse Médicale Belge*, to substitute for all the therapeutical agents hitherto employed for this disease, the *oil of petroleum*, which, according to him, instantaneously kills the parasite, while it is, at the same time, a disinfectant against the *larvæ* which are wont to be found in the wearing apparel and bedclothes. This communication has been referred to a Commission composed of Messrs. Thiernesse and Thiry.—*Glasgow Med. Journal, Jan.* 1865, *p.* 482.

68.—ON THE TREATMENT OF SCABIES.

By Dr. John Lowe, King's Lynn.

The compound tincture of iodine is the cleanest and surest cure for scabies, where the surface affected is not very extensive. It should be applied once a day with a camel-hair brush. At the first application the clusters of spots may be painted together; but afterwards it will be necessary merely to touch any fresh ones which may appear. By this method the acari are effectually sealed up, and prevented from spreading over the surface. I have used the tincture alone in the cure of scabies for the last six or seven years, and only in a few instances where the whole body was infested have I found it necessary to anoint with the compound sulphur ointment of the London Pharmacopœia. It is necessary to put in a "caveat" against applying the tincture over a too extensive surface. Where the amount of disease is considerable. it is better to take one part of the body each day, going carefully over the whole of it in succession. By this means all risk of iodism is avoided.

This plan of treatment I have found of the greatest value to those whose avocations did not permit the use of extensive inunction.—*Lancet, Dec.* 31, 1864, *p.* 754.

69.—ON THE TREATMENT OF SCABIES.

[In reply to a question of a correspondent of the 'Lancet,' Mr. T. M. KENDALL, of King's Lynn, replies that he has never found the following to fail as an immediate cure of itch.]

Sulphur combined with lime in a liquid form. It is prepared by boiling one part of quick-lime with two parts of sublimed sulphur in ten parts of water until the two former are perfectly united. It must be constantly stirred during the boiling, and when they have combined, the fluid is to be decanted, and kept in a well-stoppered bottle. Wash the body well with warm water, and then rub the liquid into the skin for half an hour. It is only needful then to wash the body well, and use clean clothes.

[Mr. G. STRUTT, of Tutbury, recommends the following ointment.]

Rub down one drachm and a half of iodide of potassium with one drachm and a half of lavender water; then add three drachms of precipitated sulphur and two ounces of lard. Mix well together, and rub well on the parts affected for three consecutive nights. On the fourth night the patient to be well washed with soap and water, and the linen changed. Of course the usual directions must be given for scalding the linen and baking the cloth clothes. When the surface is extensive, I generally prescribe the compound or simple sulphur ointment; but if both these are very strongly objected to, I have recourse to the white precipitate ointment, or a solution of bichloride of mercury in rose water. There are three or four formulæ in Paris's Pharmacologia, p. 595, vol. ii. I heard of a family some short time ago who had cured themselves with gunpowder mixed with lard.—*Lancet, Jan.* 14, 1865, *p.* 54.

70.—ON THE LOCAL EMPLOYMENT OF IODINE IN ITS PURE STATE IN THE TREATMENT OF INFLAMED SCROFULOUS CERVICAL GLANDS, AND OF INFLAMED INGUINAL GLANDS FROM SYPHILIS.

Under the above title, Dr. PRIEUR has addressed to the Academy a memoir, of which M. Ricord reports in favourable terms. The proceeding consists in applying to the enlarged glands laminæ of iodine, enclosed in a layer of wadding, where they are rapidly vaporized under the influence of heat. The iodine ought to be spread as uniformly as possible, over the half to a third, or a quarter, of the thickness of the wadding, which should be covered or fringed with a leaf of gelatine, the circumference of which adheres to the skin, and concentrates the iodine vapours to a determinate point. The apparatus is left in its place for from twenty-four to forty-eight hours, and

the result is a phlyctena filled with a thick purulent, or bloody serosity. Dr. Prieur states that he has treated in ten years about one hundred and twenty patients by this plan, and has thus caused the disappearance of more than three hundred swellings. M. Ricord, in reporting on the paper, regrets that in speaking of the inguinal swellings, the author of the memoir has not specified whether he treated inflamed glands symptomatic of infecting chancre, or simply buboes sympathetic of the soft chancre, or even virulent buboes. But the inguinal region, like the neck, is the chosen seat of strumous swellings, and often a chancre or blenorrhagia is only the occasion of the development of these enlargements. M. Ricord has himself tried the plan of Dr. Prieur in eight cases of well-marked scrofulous adenitis, and the results he has obtained have been confirmatory of the success of the treatment.—*British and Foreign Medico-Chirurgical Review, Jan.* 1865, *p.* 236.

71.—ON THE EMPLOYMENT OF BENZINE IN TRICHINIASIS.

By Professor Mosler.

In a work on intestinal worms, Professor Mosler has pointed out the poisonous influence exerted upon trichinæ by benzine. This substance was formerly obtained by distilling benzoic acid with lime, but is now procured in the preparation of coal naphtha. There seems to be no doubt that benzine destroys the trichina, but it is also poisonous to the animal infested by the parasite. It was therefore a question with Dr. Mosley, whether in the human subject, doses of benzine might not be given, which, without being poisonous to the patient, would prove a remedy for trichiniasis. After performing several experiments on the lower animals, Dr. Mosler was enabled to study the question in the human subject during an epidemic of trichinasis, which occurred at Quedlinberg last year. The benzine treatment was adopted by giving benzine ʒij. with liquorice juice and mucilage of gum-arabic, of each ʒj., and peppermint water ʒiv. Of this mixture a tablespoonful was given every one or two hours, and in this form the benzine was well borne. Dr. Mosler was satisfied that none of the patients experienced any bad effects from the remedy. His conclusions on the subject are as follows: Benzine holds the first place among anthelmintic remedies, and may be administered to the human subject with safety, in larger doses than was formerly thought possible; given in doses, which are well borne by the human organism, it kills the trichinæ in the intestinal canal, and so prevents propagation of the parasites; and therefore benzine is the only rational treatment to be employed in the trichina disease in man.—*British and Foreign Medico-Chirurgical Review, Jan.* 1865, *p.* 236.

AFFECTIONS OF THE EYE.

72.—ON TRICHIASIS.

By Dr. E. Percival Wright, Surgeon in Charge of the Ophthalmic Dispensary at Doctor Steevens' Hospital, Dublin.

[All the operations for the relief of trichiasis may be arranged under the following heads.]

1st. The removal of the offending cilia, and the extirpation of their bulbs.

2. The extirpation of the whole row of cilia.

3. The excision of a fold of the eyelids, so as to remove the cilia from the surface of the eyeball; grooving the tarsal cartilage, &c.

4. The dividing the row of cilia, with their bulbs, from the tarsal cartilage, and removing it higher up on the eyelid.

The first three are well known to British surgeons; and modifications of them have been from time to time proposed. Thus the extirpation of the whole row of cilia was suggested, as long ago as 1722, by Heister; approved of, some years later, by Cortum; was modified by F. Jæger in 1818; and, lastly, the operation was improved, in 1844, by Sir W. Wilde, who was among the first to introduce a knowledge of this operation into Britain. The excision of a fold of the eyelids, for the purpose of removing the cilia from the surface of the eyeball, will be found recommended in the works of Celsus, Actius, Paulus von Aegina; and from the days of these fathers of medicine down to the present time, some one modification or other of this operation has been in vogue; of these that of Gaillard appears to me the best.

The last method on the list—that of directly displacing the cilia, and implanting them elsewhere—was first suggested, in 1844, by Dr. Jæsche, and was, almost immediately afterwards, modified by Arlt, at that time assistant to Fischer at the School in Prague, now successor to F. Jæger in the Allgemeine Krankenhaus, Vienna; and it is this method—which Professor Arlt himself was the first to call my attention to—that I now wish to recommend very strongly to the profession. The details of this operation are very shortly these:—1. The formation of an intermarginal excision. It was chiefly in this particular that Arlt modified Jæsche's operation. Jæsche made his first incision in the conjunctiva; Arlt, on the contrary, adopted Flarer's plan of making the incision between the tarsal cartilage and the cilia bulbs, and thus leaving the tarsal edge to a great extent untouched. Flarer originally proposed this plan for the purpose of extirpating the eyelashes—first making the intermarginal

incision, then gently raising up the flap containing all the cilia and their bulbs, and then cutting this off with a sharp scissors; and, though I have seen most excellent results from the plan of Sir W. Wilde, above alluded to, yet had I to perform the operation of extirpating the eyelashes, I would be inclined to adopt the method of Flarer, as by it the bulbs of the cilia cannot fail to be eradicated. In making this incision it will be found of great advantage to insert under the lid a horn spatula (Jæger's or Saunders'); this will help to make the lid somewhat firmer, as well as put it on the stretch. An assistant should now slightly raise the lid upwards until the tarsal margin becomes exposed. The skin and the cilia are dissected from the tarsal cartilage (*vide* Fig. 1), by means of a sharp scalpel, the incision commencing at the inner angle, cautiously avoiding the lachrymal puncture. Care should be taken to glide the edge of the knife along the surface of the cartilage, so as to avoid wounding the bulbs of the cilia. 2. The next step is to disengage an elliptical flap from the upper surface of the eyelid. The first incision is to be made about two lines from the tarsal edge of the lid; the second incision is made two or three lines from the middle of the first incision, and slopes down at either side to join it (*vide* Fig. 2); this flap is then neatly dissected up, and cut off. 3. The flap containing the cilia, attached at either end to the body of the eyelid, is then freed by passing the scalpel in through the space between the tarsal cartilage and the flap with the cilia, until it makes its appearance in the upper wound; it should be

Fig. 1.

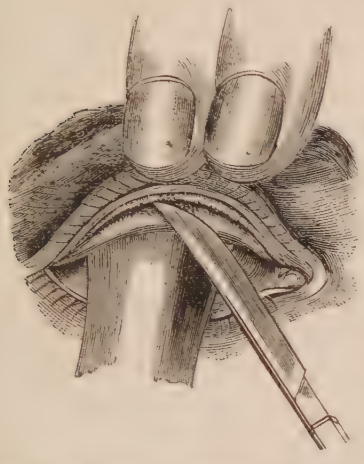


Fig. 2.

then carried along from the puncture to the outer margin, disengaging in its way any attachments, but not cutting out at either end. 4. The scalpel being withdrawn, the flap is approximated to the upper part of the eyelid by four or five stitches; a compress of charpie, with a bandage, is applied, and in three or four days the stitches may be removed; union will be found to have taken place by first intention, and after the lapse of a few weeks not only will all annoyance from the inverted eyelashes have been removed, but there will not be the slightest trace of deformity.

In very bad cases of trichiasis the shape of the elliptical flap dissected up, as described in the second stage of the operation, may be somewhat altered, and the second incision be commenced at one end of the first incision in the eyelid, and then carried upwards as high as five lines above it, and down again to join the other extremity of the same incision—thus making a flap which will have the outline of the moon in its first quarter. In all cases the depth of the excised flap must depend on the amount of the trichiasis.

The operation is most successful when performed on the upper eyelid; but this is the lid most often affected. When trichiasis occurs on the lower lid relief will be obtained by adopting some of the many methods for removing a fold of the skin, such, for example, as Gaillard's.

From the winter of 1860, when I first performed this operation, I have had many opportunities of judging of its success, and I have never yet known it fail. One drawback it certainly has, and that is, the destruction of almost all the Meibomian glands; but then this is true of all the operations for the removal of the cilia, and practically I do not find this objection one of very great importance. Another objection is, that the flap may mortify before having formed a fresh attachment; but this has never happened to myself, and depends, I think in a great measure on the care with which the operation is performed. The use of cold water fomentations are much more likely to lead to mortification of the flap than when it is dressed with charpie. [Since writing the above I have seen Dr. Waldhauer's paper in the *St. Petersburg Medi. Zeitschrift*, Bde. II., 301, in which he gives the results of 200 cases operated on in this manner; and in the great majority of these cases the results were most satisfactory.] Von Græfe's bandages here, as in all other operations about the eye, are of the greatest possible convenience, and it may not be amiss to mention that the charpie to be used is nothing but finely shreded Irish linen. On one occasion I recollect that a small slough formed on the centre of the flap, but it did not proceed very far, and on cicatrization the defect was scarcely noticeable. As a rule the eyelids are not subject

to sloughing; and if this operation be performed with a sharp scalpel, and the parts cleanly dissected, it is not much to be dreaded.

I believe that this operation of Jæsche's, modified by Arlt, has been successfully performed in this city, by Drs. Smyly and Stokes, at the Meath Hospital, and Drs. Hamilton and Symes, at Steevens' Hospital, and probably by many others; but it has been a constant source of surprise to me that so excellent a method should be so little known in this country.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 90.

73.—PHLYCTENULAR KERATITIS AND VASCULAR ULCERS OF THE CORNEA, AND ON THE USE OF THE SETON IN THESE CASES.

By W. Spencer Watson, Esq., Assistant-Surgeon to King's College Hospital, and Clinical Assistant to Mr. Wordsworth, at the Royal London Ophthamic Hospital.

[Dr. Watson tabulates nine cases of this affection.]

The cases tabulated occurred in the practice of Moorfields, under the care of Mr. Hulke, by whom they were transferred to my care for observation and treatment. The principal point which they appear to prove (so far as such a small number of cases can prove anything) is that in the more severe forms of strumous ophthalmia, with irritable vascular ulcers, when other remedies have failed, the seton forms a most valuable and almost infallible method of treatment. Another point, to which I would direct attention, is the obstinate nature of such cases, as Case No. 6, of a young girl, at about the age of puberty, in which, though the disease has been temporarily arrested again and again, it constantly recurred. The value of the seton in these cases is the more fully demonstrated, as it was not resorted to until other remedies had failed.

I do not bring the subject forward as anything novel, but simply with a view of confirming previous observations by various surgeons before me, and at the same time of directing the attention of the profession generally to a plan of treatment which appears to me to be very valuable in a class of cases which are as a rule most intractable and most distressing to the patient.

It is, perhaps, worth while to mention that the seton employed consisted simply of a thread of ligature-silk; that it was passed double through the skin of the temple, being carried in the eye of a large curved needle; and that in order to avoid any visible scar remaining, it should be passed in the upper and back part of the temporal region, so as to lie under the hair.—*Ophthalmic Hospital Reports*, 1864, *p.* 301.

74.—ON THE TREATMENT OF PROLAPSE OF THE IRIS.

By George Lawson, Esq., Assistant-Surgeon to the Royal Ophthalmic and the Middlesex Hospitals.

[When the prolapse of the iris is small and produced by a clean, sharp, but small cut near the margin of the cornea the case is almost certain to do well. The sudden exposure of the eye to a strong light is simple and may be tried, with the view of drawing the prolapsed iris back again within the chamber of the eye, but it will rarely or never succeed.]

Since the introduction into ophthalmic practice of the Calabar bean, we have at our disposal the means of causing a rapid contraction of the pupil, and of maintaining this state as long as it may be deemed desirable. This drug has been used with success in certain cases of prolapse of the iris, seen by the surgeon very shortly after the accident which caused it, and the steady and continued contraction of the pupil has succeeded, in some instances, in drawing it out of the wound; and in others, although the success has been partial, the prolapse has been diminished. As a rule, however, the patient is not seen sufficiently early to allow of this treatment, and other means have to be adopted, not for the purpose of returning the prolapse, as this a few hours after the accident is impossible, but with the view of preventing an increase of it, and of allaying the irritation which is apt to follow the injury.

Removing, with a pair of fine scissors, the Prolapsed Iris.—From the very slight amount of irritation which follows the operation of artificial pupil, when an opening is made at the margin of the cornea and a piece of iris is drawn out and cut off, it might reasonably be inferred that the snipping off of a piece of iris which has bulged through a small wound made accidentally instead of intentionally, would be followed by no more severe results. The cases, however, are not exactly parallel; in the one case, the wound is a simple incised one, made with a clean sharp-cutting instrument, its edges falling into accurate apposition, immediately after the iris has been cut off, and primary union speedily resulting; whilst in the other, the wound is nearly always a punctured one inflicted with a considerable amount of force, and producing a certain amount of contusion; hence, instead of a direct union of the edges, the wound is apt to remain more or less patulous, and after the prolapsed portion of iris has been snipped off, the constant drain of the aqueous will often cause an additional protrusion to follow, and thus render the subsequent displacement of the pupil greater than that caused by the primary injury. There are, however, cases where this line of treatment is judicious,

but they require to be selected. Those to which it might be applied are—

1st. Where the wound is small, where it has been made by a fine, sharp instrument, where there is little or no contusion of the corneal tissue, and the prolapse is not large.

2nd. In certain cases where there is a portion of iris lying between the lips of a large wound across the cornea. The removal of it allows the edges of the cut to fall into apposition, and favours primary union.

3rd. It may be often resorted to with advantage at a later period in the treatment of a case of prolapsed iris, when the protruded portion has become coated with a film of lymph, and the edges of the wound through which it bulges are evidently sealed to the iris which fills it.

[In the majority of cases of prolapse of the iris from injury, it is best at the onset to close both eyes and place a little cotton-wool or charpie evenly over the injured one, so as to form a compress, which is to be kept in its place by a turn of a roller. This often affords considerable comfort, and so long as this is the case it may be worn. The most satisfactory results sometimes follow judiciously pricking the protruded iris, as suggested by Mr. Bowman.]

The portion of iris which protrudes through the wound is usually a fold, bulged to its fullest extent by the aqueous behind, the hollow of the fold communicating directly with the anterior chamber, which it continues to do, until the opposed surfaces of the bag of iris are compressed by the falling together of the edges of the wound.

The object of the operation is twofold. 1st. By pricking the prolapsed iris, to allow the aqueous which fills it out to escape and cause it to collapse, and at the same time to permit the edges of the wound to close upon it, and come into more immediate apposition.

2ndly. To excite sufficient local action to produce adhesion of the edges of the wound to the iris and of the opposed surfaces of the iris to each other. The wound is thus closed, and the portion of iris beyond the cornea, cut off from its vascular supply, withers away.

Treatment of Prolapse of the Iris by Ligature.—In cases where a small prolapse occurs close to, or at the circumferential margin of the cornea through a *small* but *clean cut wound*, the placing a ligature of very fine silk upon the protruded iris is a very excellent mode of treatment.

It strangulates and cuts off the bulge and excites sufficient local action to close the wound, but this treatment is only applicable to a limited class of cases, as, if the wound is large,

the diminishing suddenly the size of the protrusion by a ligature soon after the accident, would either favour an additional prolapse, or else, as the aqueous was re-secreted, would allow the prolapsed iris, with the ligature, to be sucked back within the anterior chamber. The latter accident occasionally occurs in iriddesis, if the opening in the margin of the cornea is made too large.—*Ophthalmic Hospital Reports*, 1864, *p.* 227.

75.—ON THE RECENTLY IMPROVED METHODS OF EXTRACTING IN CASES OF CATARACT.

By Dr. Charles Taylor, Honorary Surgeon, Nottingham Eye Dispensary.

[In the following paper, Dr. Taylor avails himself largely of the facts detailed in a pamphlet published by Albert Mooren, M.D., *On the Diminished Risks of Corneal Suppuration after Extraction of Cataract.* No translation of this has yet been published.]

Mooren was led to conclude that the occurrence of slight iritis was the starting point of all the inflammatory phenomena which led to acute suppuration of the eyeball and consequent loss of sight, and that the inflammation always commenced in that part of the iris compressed during the exit of the lens. He says:

"One is led by these facts to consider whether it be not possible either to prevent the occurrence of iritis in extraction, or at least to deprive this inflammation of its dangerous characters. Through an exact study of my operation cases, I believed myself to have arrived at a solution of this problem. Some years ago, in two cases in which the iris was much pulled in the passage of the lens, I had cut off the injured portion as in iridectomy; and, remarkable to say, no reactionary symptoms followed. In this procedure I saw at that time only an evil, and practised it through extreme necessity; not with any idea of preventing inflammation, but only to guard against prolapse. For a long time I remained in the same view; and until I obtained additional similar experience, I gave the matter no more attention..... In March 1859, I had occasion, through the unfortunate result of two extraction-cases, to observe what an extraordinarily important part is played by the iris in the healing of the corneal flap. There came to me a big, fat, corpulent, full-blooded peasant's wife, entering on her sixtieth year, to undergo extraction in both eyes; the removal of the lenses was accomplished without any difficulty. After completing the operation, the idea struck me that this patient, on account of the great plethora of her head, might probably suffer from iritis, and the more as her enormous corpulence forbade her to remain

long at rest, and compelled her to spend part of her time sitting upright in bed, even at the very point of applying the bandage. I introduced an iris-hook under the cornea, in order to withdraw and cut of a portion of the iris; on one side the manœuvre was completed; on the other, the pupillary margin slipped from me before I could seize the moment to cut it. In consequence of the pulling which the iris had sustained, this part inflamed with such severity, that on the second day the cornea was already totally infiltrated with pus. The other eye healed without any disturbance, and the patient to this day enjoys with it unimpeachable vision.

"Three days later, the same unpleasant accident occurred to me with another patient. Here, also, the iris slipped from the hook, and the rapidly-developed iritis produced likewise destruction of the cornea. I had thus gained the knowledge that the seizure of the iris with a hook was a dangerous manœuvre, from which evil consequences might be expected; just as perfectly as the instrument satisfied every desire when the iris is immediately secured and cut, so extraordinarily dangerous is it, when the membrane slips off again, and is thus exposed to repeated traction. The application of the common iris-forceps, such as the one commonly used in iridectomy, would from time to time entail this consequence, that, in great unsteadiness of the patient, or by strong turning of the eye in an opposite direction, complete dialysis would be produced. In order to render deviation of the eye, and consequent slipping of the iris impossible, it would be necessary to hold the globe with a hooked forceps. But let us think of the extreme danger involved in such a proceeding. Nearly half of the cornea has been separated from its periphery by the section; the lens is removed; the vitreous body is only held in place by the thin posterior wall of the capsule. How light soever might be the pressure, and on whichever side it came, when the traction of the forceps was felt, bursting of the empty capsule and escape of the vitreous humour would be the first, and most natural result; at all events, to complicate the operation in such a manner for the sake of avoiding inflammation, would double the risks of extraction rather than diminish them. The procedure would be dangerous, and consequently is inadmissible. Another solution of the problem was therefore necessary.

"The first opportunity offered itself in May 1859. A woman, past seventy years of age, with faded wrinkled skin, and of such weakness that she could only walk when supported by a stick, compelled on account of great exhaustion to spend most of her time in bed, presented herself to me for extraction. Under these circumstances, it was evident that the least iritis would be followed by sloughing of the cornea; plainly speaking, the

state of things seemed to me so desperate, that I did not undertake to operate without some feeling of repugnance. I determined, therefore, to divide the operation into two parts. Acquainted with the small risk attending iridectomy, I first made this from below. The healing process was completed without disturbance, and the patient was ordered the most strengthening diet. Three weeks later I performed extraction by an inferior flap. I thus had the advantage, first, that in premature escape of the aqueous humour the iris would not be pinched between the lens and the knife; secondly, that the cataract itself had not to be pressed through a narrow pupillary opening. In this way were two conditions obviated, which in the ordinary method of operating greatly promote the occurrence of iritis; as a fact, the fortunate result entirely surpassed my expectations. The healing process went on without any signs of reaction; and, fourteen days later, the eye no longer showed a trace of vascular injection.

"In the month of June, a second eye was offered to me for extraction, in which the chances of success by the use of the usual operation would have been very small. Encouraged by the favourable result I shortly before obtained, I determined to practise again the same method. This case occurred in a lady of 76, my own aunt. The patient had been blind for several years, and showed that delicate skin upon the face and hands at which, with reason, all operators become alarmed. This sign may be observed in more than three-fourths of the instances of threatened sloughing of the cornea. First, iridectomy was performed; fourteen days later, extraction. The cornea proved to be extremely thin, and wrinkled itself upon the completion of the section, the centre and the cut edges sinking in: on the third day, the eye was opened. An insidious iritis had developed itself without pain, and had produced grey exudation masses in the pupillary space. The whole treatment consisted in the administration of decoction of cinchona. Three weeks later, all inflammation had subsided; but under its influence the pupil had become in great part closed, and the patient had only a very small amount of vision.

"A long illness that shortly afterwards befell me, did not allow me until autumn to think of opening the pupil by operative means. The patient underwent the renewed iridectomy without any inflammatory reaction. Four weeks later, with $2\frac{1}{2}$ convex, she read No. 3 of Jäger's test-types; and this lady still enjoys such excellent vision, that at present she occupies nearly all her time in reading and writing. This case proved to me, more than the former had done, that the supervening iritis which, under other circumstances, would have totally destroyed the eye, had been deprived of its dangerous characters.

"Likewise, it seemed established that, by this method, the occurrence of disturbing processes might be prevented. It remained, therefore, to determine the limits within which it should be preferred. Speaking for myself, I could not but arrive at the feeling, that extraction, as it had heretofore been taught and practised, was no longer to be desired in opposition to my method of diminishing its dangers. *I saw my method, not only as a supplement, but as an absolute necessity in extraction.*"

I have preferred, in the above extract, to give Dr. Mooren's reasons for the adoption of a preliminary iridectomy as nearly as possible in his own words. He does not recommend his operation, however, to the entire exclusion of the ordinary method; but only in cases where, from the state of the patient's health, or the condition of the eyeball itself, the ordinary operation is almost certain to fail. Thus, he always makes a prior iridectomy, in cases of great age, complicated with general debility; in cases of disease likely to interfere with, or prevent, the healing process, such as severe cough, asthma, persistent vomiting, or affections which preclude long maintenance of the recumbent posture; a thin, silky, debilitated state of skin; or such an amount of constitutional cachexia as would lead the surgeon to anticipate disaster as the result of any operation.

The existence of pupillary adhesions, a diseased or deteriorated condition of the lens or eyeball itself, are also indications for his method of operating. Thus, if the pupil be thoroughly dilated with a drop of atropine solution, the lens will readily pass through the pupillary aperture, and there is so much the less indication for a preliminary iridectomy. Again, if the cataract have a small nucleus, surrounded by much cortical matter in coherent masses, the escape of some portions of this soft matter into the anterior chamber is so apt to provoke an insidious iritis, with subsequent suppuration of the globe, that, in this condition of the lens, a careful prior iridectomy is the only safe method of operating. The worse the patient's condition, the longer must the interval between the iridectomy and subsequent extraction be. In Mooren's cases, a fortnight or three weeks only was allowed to elapse; but six weeks is the shortest interval now adopted. I may mention that Mooren's practice, with the exception of Von Gräfe's, is the largest in Prussia, if not in the whole of Germany; he having had 578 operations in 1860, and 622 in 1861.

Since 1859, up to the period of the publication of his pamphlet (1861), Mooren practised his method in every case where the ordinary operation appeared certain to fail, with such brilliant results that he feared the incredulity of his readers; and, instead of contracting his narrative to a simple percentage

proportion between successful and unsuccessful cases, thought it necessary to verify his statistics by giving the names and addresses of all the patients operated upon in this manner. Here are one or two average specimens.

"Frau Greis, from Eberfeld. Iridectomy, October 14th. Extraction, October 22nd. Very pasty individual, with thin skin, and very insufficient dilatation of pupil. She is so asthmatic, that she seems every moment on the verge of suffocation, and is always, therefore, in the highest state of agitation. On completing the section, the cornea wrinkled itself very much; the removal of the lens was attended by very great difficulty, both from the unsteadiness of the patient, and also from firm adhesion between the cataract and posterior capsule. Tedious healing. Granulation, with much spasm of the lids. Recovery, with tolerable vision.

"Frau Berger, from Viersen, only fifty years old; but a picture of weakness,, decrepitude, and anæmia. The left eye had undergone reclination elsewhere, and the uprisen lens irritated the eye. I attempted its removal. Upon introducing the knife, however, the aqueous humour and the vitreous humour of like consistence trickled forth, and the sclerotic fell into wrinkles. These conditions, together with circumscribed capsular opacity of the right eye, led to the conclusion that the vitreous humour was perfectly fluid in the right eye also. Iridectomy, Sept. 8th. The cornea wrinkled itself; the effused blood required fourteen days for its absorption. The patient was so feeble as to be kept in bed for three weeks. She left her bed for the first time on the day before the extraction, which was performed on October 1st. Full diet. A secondary cataract remained, and, together with an insidious iritis, produced an obstructed pupil. In the beginning of December, therefore, a second iridectomy was required. Very satisfactory vision. In both the operations, the cornea fell completely; and the patient's condition of marasmus was so extreme, that my assistants, and Dr. Ruschhaupt, who witnessed the operations each time, earnestly endeavoured to dissuade me from performing them."

Without quoting further from Dr. Mooren, I will now detail two or three cases in which I have performed his operation; which will, I doubt not, be all the more satisfactory, as I shall be able to show you the patients.

Case 1.—Ann Allwood, of Mansfield, a feeble, cachectic individual, in her fifty-sixth year, consulted me in May last year on account of an amaurotic condition, with partial lenticular opacity of the left eye. On ophthalmoscopic examination, I found the retina extensively diseased; the vitreous body fluid, and crowded with shred-like patches, which floated freely to and fro; and her vision so much impaired that she required a guide

in the street. The right eye had been useless for some years; having, in addition to a perfectly fluid vitreous body, indicated by a remarkably tremulous iris, a hard calcareous cataract. As she had some perception of light with this eye, I thought it possible that she might be benefitted by extraction; and, accordingly, proposed the operation. I also considered this an excellent case to test Mooren's method, as the patient's health was very bad, the eyeball extensively diseased, and an ophthalmic surgeon had assured her that any attempt at extraction would be followed by suppuration of the globe. Iridectomy was performed early in June; extraction three weeks later. The patient recovered without a bad symptom. Ten days after the operation, there was scarcely a trace of vascular injection; and it was only on careful inspection that the fact that she had recently undergone extraction could be ascertained. After the operation, she had increased perception of light, but no useful vision. The ophthalmoscope revealed extensive disease of the retina and vitreous body.

Case 2.—Alice Duffield, Nottingham, a decrepid, pasty individual, in her seventy-seventh year, with thin wrinkled skin, and so feeble that she had required the support of a stick for some time past, had been in very bad health, and had suffered three slight paralytic attacks. Her medical attendant assured her that she was not fit to undergo the operation. I thought this a good case for Mooren's method; and accordingly performed a superior iridectomy on the left eye early in August last year, extracting a month later. The patient was very unruly; and the cornea was wrinkled and depressed in the centre. The lens was extracted without difficulty. She was perfectly comfortable, and without a bad symptom for five days; when, contrary to orders, she got up, removed the pad, and exposed her eye by looking out of an open window. Acute catarrhal ophthalmia, culminating in a very sharp attack of iritis with closed pupil, was the result. Three months later, therefore, when the eye was quiet, I performed a second iridectomy, opening up the pupil, and restoring good vision. Eighteen months previously, when she was in much better condition, I performed ordinary extraction on the right eye. There was an escape of fluid vitreous humour, and considerable prolapse of the iris, followed by general iritis, turbidity of the humours, and the establishment of a corneal fistula through which the aqueous humour drained slowly for months. The eye was, however, ultimately saved, after she had undergone considerable pain and discomfort.

Case 3.—Joseph Falconbridge, a very feeble patient, wasted by poverty and disease, in his sixty-eighth year, was suffering from chronic psoriasis, with patches of ulceration on the skin,

and had recently been an inmate for three months of a general hospital on account of chronic disease of the bladder, which necessitated his rising several times each night. He was also afflicted with ingrowing lashes, which had caused a patch of ulceration on the lower half of the cornea of the left eye, which was the site of a hard senile cataract. This was altogether one of the most unfavourable cases for operation that could well be conceived. Iridectomy was performed on July 2nd, 1863. Owing to a very prominent brow and great emaciation, the eyeball (very small and hypermetropic) was so sunken, that I had great difficulty in introducing the iridectomy-hook. Extraction was performed three weeks later. On opening the eye on the seventh day, the corneal surface was covered with pus, the result of severe conjunctivitis, caused apparently by the irritation of the lashes. The wound was quite healed; the cornea perfectly clear. The conjunctivitis yielded in a few days; and the patient recovered with excellent vision.

Case 4.—Mary Rame, a feeble patient, with thin, wrinkled skin, in her sixty-fifth year, was much reduced by poverty, confinement, and depression of spirits, the result of blindness caused by hard senile cataracts in both eyes. Iridectomy was performed in the left eye early in August last year; extraction six weeks later. The lens was very large, and surrounded by soft coherent matter, some of which floated into the anterior-chamber, and which, owing to her unsteadiness, it was impossible to extract. She did exceedingly well for five days; when, owing to the irritation of the lens-matter or to some unexplained cause (I did not see her after the operation), severe iritis was developed, ending in closed pupil. I have not yet had an opportunity of remedying this defect; but do not doubt that a second iridectomy will restore her excellent sight.

Case 5.—Ann Fox, aged sixty-five, with hard senile cataracts in both eyes, was in reduced health, owing to depression and confinement, the consequence of blindness; and very obese, so that she could not maintain the recumbent posture for any length of time. She would probably have done well with the ordinary operation; but, as she was excessively anxious as to the result, I gave her the choice, and she selected the safer procedure by Mooren's method. Iridectomy was performed on the right eye early in August last year; extraction six weeks later. She recovered without the least unfavourable symptom; has excellent distant vision, and reads diamond type.

I found in these cases that the coloboma iridis was completely concealed by the upper lid; that the pupil was about the normal size; that it was influenced by a strong light, dilated by atropine, and contracted by Calabar bean. The patients did not complain of dazzling; and the only inconvenience attending

the operation was its division into two parts, separated by a somewhat long interval.

Mooren publishes sixty selected unfavourable cases of which the two I have quoted are fair specimens. These cases, he states, would have been lost with certainty by any other proceeding—an opinion confirmed by Dr. Ruschaupt and his two assistants, Drs. Meisner and Josten. The operations were attended by the remarkable result of only two failures. In one of these, when not under the doctor's care, the eyelashes became inverted, and raised the corneal flap; in the other, there was at the time of the operation, complete retraction of the corneal flap—in itself, tantamount to a local mortification. Prior to the adoption of his method, eleven failures in ninety-seven operations, or more than one in ten, was the average of failures with ordinary cases presumably favourable in the hands of the same operator.

The cases most likely to fail by the ordinary, or indeed any method, are those where the cornea is extremely thin—a condition mostly observed in cachectic and aged individuals; a condition, moreover, which it is impossible to diagnose with certainty prior to the completion of the section. We may always, however, suspect its existence when the skin is extremely fine and thin. There are three degrees of this condition; the first, marked by a central depression of the cornea, often with transverse wrinkling immediately on completing the section; a second, where the shrinking is such as to cause withdrawal of the flap; and a third, where there is more or less positive retraction. This complete retraction implies absolute loss of the elastic properties of the cornea; and, whenever it exists, leads to total destruction as soon as the ordinary reaction attains the height of an inflammatory process.

My experience of Mooren's method is, that there is much less danger of subsequent inflammation; and, that, when this does occur, it is robbed of its dangerous properties. The preliminary iridectomy affords a permanently large pupil, through which the capsule of the lens is very readily incised, and through which the lens itself passes with the greatest ease. After the operation, the edges of the corneal flap are most easily replaced, and there is no chance of that *bête noir* of extraction, prolapse, to disturb the healing process. If the inferior section (the one always used by Mooren) be adopted, the remains of the lens are removed more easily; any portions left, too, are less liable to press upon and irritate the iris, when this membrane offers to them a diminished bearing surface.

There are three objections to Mooren's method. The first is, the deformity occasioned by the removal of a portion of the iris; the second, the liability of the patients to unpleasant dazzling through the enlarged pupil; and the third, the inconvenience

attendant upon the division of the operation. The first two cease to be objections when the upper section (the one which I have always adopted) is used. The last is indeed a most serious contra-indication and one that must necessarily limit the operation. This is so palpable a fault in Mooren's operation, that in the two other improved methods of extracting to which I have briefly to direct attention (Jacobson's and Schufte's), the iridectomy and extraction are combined. Jacobson makes the iridectomy the last step of extraction, and gets rid of the obvious dangers of manipulating the globe after incision of the cornea, pointed out by Mooren, by doing the whole operation when the patient is profoundly under chloroform. He, like Mooren, by continually inspecting the healing process after ordinary extraction, established the fact that the portion of iris corresponding to the flap, and compressed by the lens in its exit, is that in which inflammation always commences. This is the centre of all risk to the eye; and, to be danger-free, this must be got rid of.. He keeps his patient for twenty-four hours without a large meal, and for some hours without solid food. He pours chloroform on a compress with a wax cloth back; holds it at a little distance, until the fauces and larynx have become accustomed to the vapour; and then moulds its edges (after a fresh supply of chloroform (to the face, so as to admit very little air, and to push anæsthesia rapidly. When the patient is nearly fit for operation, the compress is somewhat removed, and more air given with the last portion of chloroform. The patient being in such a condition that the contact of instruments produces no action whatever of the muscles of the globe or lids, the latter are held apart by an assistant, and Jacobson proceeds to make an inferior flap, with the line of incision not in the transparent tissue, but in the vascular limbus corneæ, so as to get a larger opening for the lens, and to place the wound nearer than usual to the materials for repair. For a large or externally softened lens, he makes his puncture and counter-puncture half a line below the horizontal meridian; for a smaller or hard lens, somewhat lower. This being premised, and also that he selects a knife of a breadth corresponding to the semi-diameter of the cornea, he proceeds, sitting on the edge of the bed in a convenient position, to seize the conjunctiva and subconjunctival tissue with toothed forceps, below and to the inner side of the cornea; with the disengaged hand he enters the knife at the junction of the cornea and sclerotic, carries it quickly through and out at a corresponding point, and pushes it gently on with its edge inclined slightly towards the iris, so that nearly all the external wound will be in the conjunctiva. The knife, being carried on as far as convenient, is gently withdrawn, leaving a central isthmus undivided. If the blade be too narrow, this-

isthmus will contain a bit of cornea; but if it be of right width, it will consist of conjunctiva alone. Von Gräfe's fleam-shaped cystotome is then introduced, and the capsule freely opened; and then the isthmus is divided; its corneal portion, if any, with the point of the knife, in the line of the original incision; and its conjunctival portion with scissors, two lines farther down on the globe. The lens is started by digital pressure through the lids, directed in the first instance from above backwards and downwards, so as to make it rotate and present its lower edge at the pupil; and any *débris* are eased out with the scoop in the most delicate conceivable way. The lower segment of iris is then drawn out with proper forceps, and cut off close to the ciliary margin. The conjunctival space and the line of incision are cleared of blood and coagula; and the lids are closed. If the cornea be in right position, Arlt's compress is applied immediately; but if it be depressed, Jacobson waits from five to thirty minutes for a resecretion of aqueous humour to elevate it. The operation being a bloody one, there is a little delay between its stages, on account of conjunctival bleeding obscuring the parts and requiring removal. Between each two stages, the anæsthesia must be noted, and, if necessary, more chloroform given; the lids being, in the meantime, closed and supported by a ball of charpie and gentle pressure. No step must be taken without certainty that it can be completed with passive ocular muscles. If vomiting occur, the lids must be closed, supported by a ball of charpie and the hand; and the patient raised to a sitting posture, to vomit; then laid down again, and more chloroform given. The anæsthesia is to be maintained until after the application of compresses, which are applied in every case to both eyes.

For twenty-four hours after the operation, each patient has the undivided attention of a nurse, who anticipates all his movements, supports his eye if he should vomit, and so forth. If any pain, or especially any *heat*, be felt in the eye, iced poultices are applied over the compresses, and changed every few minutes for hours together. If this be insufficient to give speedy relief, and if the patient will bear depletion, four leeches are applied in front of the ear, and each replaced by a fresh one as it falls, so as to take blood continuously until benefit is derived. From forty to eighty leeches have been used in this way to a single case; and this treatment is had recourse to promptly, before mischief is allowed to make head. Jacobson does not hesitate to open the eye for inspection on the second day, and, after the third, commences the systematic instillation of atropine, to prevent adhesions between the pupillary margin and the coagula left for absorption after iridectomy. Jacobson claims ninety-eight cures in one hundred operations, presumably

ordinary cases. I have not tried his method, as I conceive that chloroform vapour, concentrated to the required extent, must of itself entail some risk, especially in patients whose health is such as to render the ordinary operation inadmissible. Again, I think escape of the vitreous humour would be favoured by the position of the incision; while the excessive attention to minutiæ, the risk of vomiting, and the extraordinarily careful nursing required, are all such self-evident contraindications, that I need not dwell upon them. None of these, however, apply to Schufte's, or more properly Waldau's, method of scooping out grey cataracts—an operation which, first received with caution, afterwards, owing to several failures, viewed with disfavour, is now rising so rapidly in estimation as to threaten, in the words of Mr. Critchett, to supersede the old method of flap-extraction altogether. In this operation, the lids are kept apart by the ordinary wire speculum; the operator then fixes the eye with a pair of forceps; and, with one of Jäger's bent iridectomy-knives, proceeds to incise the cornea close to the sclerotic, so as to form a curved slit from three to four lines in extent. With a hook or forceps, a portion of the iris is then drawn out, and about one-fifth of its extent removed. It is well, in doing this, to leave a slight belt at the ciliary margin, so as to afford support to the vitreous, and prevent its escaping or prolapsing. The capsule of the lens is then freely opened, and any superficial softened matter coaxed out with the scoop of the ordinary curette. Critchett's vectis-spoon is then glided between the posterior surface of the cataract and the hyaloid membrane. As it passes behind the equator of the advancing lens, the handle is slightly sloped backwards; and, when its centre has reached the posterior pole of the lens, with a gentle lever movement it is cautiously lifted into the anterior chamber, and extracted. The greater part of the cataract is removed at once. Any remaining portions, however, must be carefully fished out with the spoon; while the globe is lightly rubbed with the lids from time to time, so as to wash any fragments concealed behind the iris into the pupillary area. Critchett's spoon brings away the cataract, or, at any rate, its firm nuclear portion, unbroken, and does not take up more room than a thin metallic layer. The introduction of this instrument has greatly facilitated the performance of Schufte's operation, and will render it much more general. It also enables us to deal with all varieties of cataract, however dense. The risks attending this method are, that the hyaloid may be ruptured, and the vitreous allowed to escape. There is also danger of bruising the iris between the cornea and spoon; and also that fragments of the lens may be left in the anterior chamber, probably concealed behind the iris. Any of these accidents may retard

the progress of the case, or even seriously compromise the eye. If they are avoided, however, and there is much less risk of such accidents since the introduction of Mr. Critchett's spoon, there can be no question that the patient's chances with a comparatively small elastic slit in the cornea are far greater than with a large semicircular flap. Scarcely any after-treatment is necessary, and the eye need not even be closed. There are many objections to the old scoop with flat bottom and steep edges, originally used by Schufte. It was apt to become entangled in the substance of the lens before it had passed behind its posterior surface; to break it up, and push portions into the space behind the iris, where they excited destructive inflammation. Hard senile cataracts also could not thus be removed.

These facts will perhaps account for the ill success which has attended this operation in the hands of many surgeons. Mooren adopted it in thirty-two cases, with the decidedly unfavourable result of losing ten eyes. He also states that other surgeons were similarly unfortunate. A friend of mine performed the operation in two cases—in both the globe suppurated; and an eminent London ophthalmic surgeon informed me that he had tried it in three cases, the eye being lost in each. The only case in which I have performed the operation was one of traumatic cataract, in which, owing to the development of glaucomatous tension, it became necessary to perform iridectomy; and I subsequently extracted the lens with Mr. Critchett's spoon. In spite of the above facts, Schufte's is just now the operation *par excellence* at Moorfields; and I do not doubt that it will ere long occupy a similar position elsewhere.

I must not omit to mention that linear extraction with and without iridectomy, is fast superseding the old operation of solution by needles, drilling, and discission, in a number of cases of fluid and semifluid cataracts. There is no risk of escape of the vitreous humour, collapse of the globe, or hemorrhagic effusion; and the results are, as Mr. Hart observes, in some respects incomparable. Of the various modifications of this operation, none yield such speedy and charming results, as Mr. Teale's revival of the ancient Persian method of extraction by suction. The operator must first, with two needles, tear up the anterior capsule, so that it may curl back from the area of the pupil, and be lodged behind the iris. Then, opposite the margin of the fully dilated pupil, make an incision in the cornea with a broad needle; through this opening, the suction-curette is introduced, and is gently buried in the opaque lens, which is then extracted by suction. Mr. Krone makes a very useful valve instrument for this purpose; and Mr. Weiss has two—one with a valve, and the other with a mouth-piece. I prefer the latter. Great care is required, to avoid injuring the pos-

terior capsule. This instrument may be also used in cases where extraction by flap is contraindicated, such as *e.g.*, diabetic cataract. The patient sees after the second or third day; and moderated light may be admitted in forty-eight hours after the operation.

In conclusion, I may perhaps be permitted to mention that I have endeavoured to simplify the ordinary operation of extraction by semicircular flap, by carefully dividing the capsule with a fine cutting needle, as the first step of the operation, when the pupil was fully dilated with atropine; extracting as soon as the few drops of aqueous evacuated had been resecreted. I have thus avoided what always appears to me a difficult and dangerous part of the operation—incising the capsule when one-half of the cornea is converted into a flap; the patient's eye rolling in all directions; the iris in contact with the cornea; and the pupil, *in spite of atropine*, reduced to a point. The risks of pressure in this condition almost preclude the possibility of fixing the eye; and the difficulties of incising the capsule freely and carefully, the dangers of scratching the iris, obscuring the operation with blood, oppressing the eye, breaking up the lens, and thus exciting the first steps of what may prove a a destructive inflammatory process, are self-evident.

Since the above was written, I have had further opportunities of testing both Schufte's and Mooren's operations, with excellent results. I may also mention that, at the recent Heidelberg Ophthalmological Congress, Professor Jacobson stated that, in 140 operations he had only lost three eyes. In one remarkable case, a patient, aged eighty-five, was kept narcotised for three hours, and inhaled upwards of six ounces of chloroform. In an animated discussion which ensued, concerning various points connected with extraction, the general feeling appeared to be in favour of iridectomy only in special cases such as I have pointed out above. Von Gräfe also expressed an opinion contrary to the generally received notion, that of two cataracts the less ripe one afforded the best results from operation. Mr. Critchett, at this meeting, also stated that he had met with such success with Schufte's method and his own modified spoon, that he now never thought of performing the usual flap operation.—*British Medical Journal, Nov.* 12, 1864, *p.* 539.

76.—EXTRACTION OF CATARACT AND FORMATION OF ARTIFICIAL PUPIL IN A PATIENT WITH THE GLOBES OTHERWISE DISEASED.

By James Vose Solomon, Esq., Surgeon to the Birmingham and Midland Eye Hospital.

[The patient was a comb-maker, forty-five years of age. He

had anchylosis of the cervical vertebræ. His *left* eye had been injured eight years ago; it was blind, disorganized, and staphylomatous from past choroido-scleritis.]

The right eye had been diseased a year only. It presented very advanced tissual changes from sympathetic irido-choroiditis; the cornea was bright, and surrounded by a large ciliary venous zone; the iris, which was bossulated throughout, nearly touched the arch of the cornea. The whole circumference of that membrane presented numerous and complex anterior synechiæ. The pupil was closed and adherent to the lens capsule. The vision was limited to a perception of shadows, which was more vivid than I have ever before met with. The patient, who had been addicted in his younger days to pugilism (which probably gave rise to the cervical anchylosis), was in excellent health.

Operations, August 15th. The disorganized eye was enucleated; and a narrow bit of iris was taken by the canular forceps from the right, in order to ascertain the condition of the lens, which it was believed might perchance be transparent, as his perception of shadows was unusually good. The lens proved to be opaque.

Operation, August 27th. The eye having recovered, the lens was extracted to-day by the lower section, the knife being carried through the sclera, and behind the iris. The cataract was easily removed, and without loss of vitreous, by a hook. It was a dark amber lens. A beautiful black pupil, with vitreous distending it, left nothing to be desired by the operator. The eye was therefore quickly closed, and dressed with jeweller's cotton wool, supported by a few turns of a roller.*

Sept. 7th. The wound is cicatrised, the pupil is well defined and open; but behind it are shreds of capsule, some soft lens matter, and lymph. The vision remains as before treatment. Tension less than normal (—T1).

Sept. 21st. The anterior chamber, from recession and flattening of the iris, is enlarged; the inference is that a communication subsists between the two aqueous chambers, although the fundus cannot be illuminated.

Operation. A broad cutting needle was inserted through the upper circle of the ciliary sclera, and brought out in front of the iris, when with a sharp backward stroke the membrane was severed, and a good pupil, distended by vitreous, was obtained.

Sept. 27th. There is a still further improvement in the

* In extraction by flap, and in cases of iridectomy, I have found this kind of dressing to supersede all others. The eye can be gently compressed without discomfort to the patient. A roller of flannel canvas ("domet") forms an admirable compressive bandage. The value of well-applied compression has been specially pointed out by Dr. Von Gräfe. *Vide Ophth. Rev.*, vol. i., pp. 324–5.

dimensions of the anterior chamber and pupil, but vision is not commensurately advantaged.

October 7th; operation. The pupil is drawn downward and closed, the upper border being well defined. The iris has lost its fibrillous character and become lighter in colour. It was therefore determined to make an artificial pupil, with a penetrating crochet iris-hook (Bowman's). A small puncture having been made slowly through the upper corneal circle without loss of aqueous, the hook was introduced, and an equilateral, triangular pupil was made, the apex of which became united to the point of puncture. The base measured about two lines.

November 22nd. The fundus can now be, for the first time, focused. The limits of the optic nerve are ill defined; there is gray atrophy. The tension of the globe is normal, chronic enlargement of the ciliary veins still exist, but there is no evidence of irritation from the surgical treatment. With a three-inch convex glass, features are distinctly seen at nine feet; at the same distance he counts fingers, but they cease to be visible when separated to a greater extent than nine inches, so much contracted is the visual field. The poor man's inability to rotate the head, in consequence of the anchylosed condition of the spine, and his amblyopic and contracted field of vision, render fixation very slow.

I am induced to report this case, from the very unfavourable results which have generally attended, in English subjects, the extraction of cataract, or the formation of an artificial pupil, where the same tissual changes have been present as were observed in the instance here given.

Having satisfied myself, by examination, that the vitreous was, in all probability, of good consistence, it occurred to me that liability to ophthalmitis, or shrivelling of the corneal flap, might be lessened, provided, in the performance of extraction, I abstained from interference with the disorganized iris, and made my section through the sclera. In considering the position of such section, it must be remembered that the iris was bossulated and united near to its periphery by numerous morbid adhesions to the cornea—conditions which permitted a wound to be made much nearer to the sclero-corneal union than would have been practicable in the healthy eye. The passage of the knife between the front of the lens and the posterior surface of the iris, by separating intimate adhesions and opening the capsule, must have materially assisted the extraction of the cataract, and so promoted early union of the flap. Bowman's hook being of small size, and furnished with a sharp-cutting inner edge, affords a most useful instrument in the case under consideration. It may be thrust through any selected point of the iris; its cutting inner edge is well adapted for the division

of the pigmentous membrane which is frequently found behind the inner ring of the diaphragm, forming a bond of union with the lens capsule.

The advantages of making the section through the posterior aqueous chamber (a method practised by Whyte, in 1801, for the extraction of cataract, uncomplicated by other intra-ocular disease) appear in cases of annular synechia to be as follows:—(1) An easy release of the lens from its adhesions; (2) the formation of the flap, edged with a membrane (the sclera), more favourably placed for nutrition than the cornea; (3) a diminished liability to destructive irido-cyclitis, by non-interference with the diseased iris until the eye has recovered from the cataract operation.

The theoretical objection to scleral section consists in the greater probability of loss of vitreous. As, however, it is scarcely possible to obtain less success than has hitherto attended the plan of performing iridectomy and extraction on the same day, I feel justified in suggesting to the profession a trial of the method here described, with a view of determining its value, as compared with other plans of treatment.—*Ophthalmic Review, April* 1865, *p.* 28.

77.—REPORT ON CASES OF AMAUROSIS FROM INTRA-CRANIAL CAUSES, IN WHICH ONE EYE ONLY WAS AFFECTED.

By Jonathan Huthinson, Esq., one of the Editors of the Royal London Ophthalmic Hospital Reports.

The ophthalmoscope has enabled us to separate into different groups all the various cases of defect or entire loss of sight resulting from changes in the deep tissues of the eye. It has, however, left us still a considerable group of cases, which we are obliged, for want of a better term, to put under the old name of Amaurosis, in which the source of damage exists not in the eye, but in some part of the visual nerve—apparatus within the skull. Of these cases, by far the majority exhibit, by the ophthalmoscope, only anæmia and subsequent wasting of the tissues of the optic nerve itself. In some we observe swelling of the optic nerve (neuritis), and a proof of some pressure upon it in the fact that the veins are dilated and tortuous; but the common condition is that known as white atrophy. In some we suspect the existence of a tumour in the brain or cerebellum, affecting the optic centres or the optic nerve tracts either by reflex influence or by direct pressure. In others some occult change is probably taking place at the nuclei of origin of the nerves (so called tobacco amaurosis). In others the sexual organs appear to exercise some reflex influence upon the func-

tion of sight. In others, again, we have the history of blows on the head, or of cerebral concussion. In a few, constitutional syphilis, either acquired or inherited (very rarely the former), induces in some unexplained way entire loss of sight, without any morbid process in the eye itself. In a few, and chiefly in the aged there is reason to suspect ordinary cerebral apoplexy, or it may be softening of the brain. Lastly, in a very few the the suddenness of the attack, and the fact of the co-existence of valvular disease of the heart, leads us to infer that embolism of the retinal artery has occurred.

All these possible causes of intra-cranial amaurosis, excepting the last (embolism), are likely to affect, more or less, both eyes at the same time. For anatomical reasons it is difficult to understand how any disease, whether acting by reflex influence or by direct disorganisation, could, if situate behind the optic commissure, affect one eye only. One eye may suffer chiefly. or one eye may become blind before the other, but that one eye should become utterly blind, and its nerve disc white and atrophic, whilst the other enjoys its perfect function, and presents, on ophthalmoscopic examination, no morbid condition whatever, is inexplicable, excepting on the supposition that the nerve trunk anterior to the commissure, or the artery which supplies it, is the part involved. It is just possible, however, that we may be proceeding a little too fast in our supposed possession of anatomical knowledge, and that there do exist cerebral nuclei for the two eyes, disease of one of which could involve one organ only. To effect this it would of course be needful for the fibres which crossed at the commissure, to cross back again in the mesocephalon.

In practice, cases of complete, monocular amaurosis, of the kind alluded to, are very infrequent. Through a period of about seven years, during which my attention has been alive to the peculiar interest of the subject, I have been able to collect very few. These I now propose to group together, in the hope that, by their investigation in juxtaposition, some light may perhaps be thrown on a very intricate subject.

It is almost needless to remark that cases recorded without ophthalmoscopic examination are of no value whatever in this inquiry. We meet with cases of monocular blindness almost every day, but with only very rare exceptions indeed we are able in such, by the ophthalmoscope to find adequate explanation in the state of the deep structures of the eye itself. A few of these cases, however, those for instance in which large retinal apoplexies occur only in one eye, and in perfectly healthy patients, or in which large extravasations take place into the vitreous body without assignable cause, are not quite without their interest in relation to our present subject. They at least

prove the possibility of extensive unsymmetrical hemorrhages in patients who seem healthy at the time, and in whom nothing further follows. Similar extravasations into the optic nerve sheath might produce the exact condition to which we refer, and it is probable that in some cases such is the fact.—*Ophthalmic Hospital Reports*, 1864, *p.* 235.

78.—ON THE RADICAL CURE OF EXTREME DIVERGENT STRABISMUS.

By James Vose Solomon, Esq., Surgeon to the Birmingham and Midland Eye Hospital, formerly Surgeon to the Birmingham General Dispensary.

[A case is one of Extreme Divergent Strabismus when the patient is unable—let his muscular efforts be ever so great—to do more than bring his eye to the centre. In the following paper the author confines himself to the consideration of instances where the deformity has had its origin either in a too free division of the tendon of the inner rectus muscle and the subconjunctival fascia, by Dieffenbach's operation for squint; or in paralysis of the inner rectus, unconnected with tumour of the orbit.]

The indications to be fulfilled by any proceeding undertaken for the radical cure of *Extreme* Divergent Strabismus, when a result of previous tenotomy of the inner rectus, are :—To give the eye a central position ; to restore the length and the proper point of attachment to the adductor muscle, thereby insuring balance of the opposed muscular forces and *permitting normal convergence.*

The division of the attachments of the inner rectus, with its conjunctiva and subjacent fascia (in a manner to be presently described), and of the tendon of the external rectus, suffices to place the eye-ball in the axis of the orbit.

The great difficulty consists in protecting and fixing the globe until the inner rectus obtains a proper attachment to it. Careful study of the subject led me to institute in persons who are in good health the following plan.

Anæsthesia having been induced by chloroform, and the lids separated widely by a self-acting speculum, the first step of the operation is commenced by making a flap of the parts which contain the inner rectus. To effect this a rather long perpendicular incision is made at about half a line from the inner margin of the cornea, down to the sclerotica ; with curved scissors the inner side of that membrane is dissected bare. In order to make the flap come forward and over the cornea, two short

incisions are practised through the extremities of the first incision, towards the nose. (Fig. 1.)

(2nd Step.) As soon as it is found that the flap will reach, on being stretched, the vertical diameter of the cornea—when the eye-ball is in the axis of the orbit—the external rectus is divided by a rather long perpendicular incision, just behind the insertion of its tendon.

The conjunctiva, which intervenes between this incision and the outer margin of the cornea, is reflected forwards, *and made to form a second flap.* (Fig. 2.)

(3rd Step.) The two flaps are united over the vertical meridian of the cornea by sutures, three of which are generally sufficient, one in the centre, one just above and one below the upper and lower margin of the cornea. (Fig 3.)

Fig. 1.

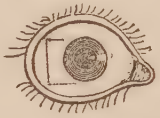


Fig. 2.

Fig. 3.

The eye now occupies the axis of the orbit; the inner rectus is stretched to its utmost limit, and the tendon placed over the most favourable site for adhesion to the sclerotica; the external rectus retracts, and if the power of moving the globe inwards and outwards be tested at this stage, it will be found still to exist, but to a slight degree only, the antagonism being about equal.

The *after treatment* aims at securing the immediate union of the inner flap, and the preservation of the nutrition of the globe. These indications are fulfilled by keeping both eyes closed, and with the temples covered for about ten days by jewellers' cotton wool. The central suture is removed on the second or third day so as to obviate *corneitis.* Inflammatory chemosis, should it occur, must be met by the application of two, three, or four leeches to the temple, and pain in the eye by the internal use of morphia. If there be much puro-mucous discharge the local use of astringents is necessary. At the end of three weeks the conjunctiva should be trimmed of any uneven processes.

I have practised this method for upwards of four years with great success.—*Mr. Solomon's Paper on the Radical Cure of Extreme Divergent Strabismus, read before the British Medical Association at Cambridge, Aug.* 1864.

SYPHILITIC DISEASES.

79.—ON THE THERAPEUTICS OF SYPHILIS.

[Gonorrhœas, simple ulcers, and soft, suppurating sores form by far the majority of venereal disorders; and mercury is unnecessary in their treatment. It is not, however, of these that the present paper treats, but of syphilis proper.]

Taking syphilis as a whole, there is no known remedy equal to mercury; none so reliable; and to certain complications, such as iritis, syphilitic orchitis, &c., this applies even more strongly. The stages in which it is of doubtful efficacy are the primary and tertiary. Of the forms, the pustular syphilides are the least amenable to it, and the scaly, chronic papular, and certain forms of chronic superficial ulceration the most so. For the tertiary stages, marked by diseases of bone and periosteum, unhealthy suppurative sores on the integument, the iodide of potassium is the best remedy. The above may, we think, be taken as a correct expression of the views entertained by the more moderate section of mercurialists.

[The writer considers that sufficient attention is not given to hygiene. The patient should be warmly clad; live upon a good but plain diet; take moderate exercise in the open air; use warm baths occasionally; and avoid stimulants unless specially indicated. The system must never be depressed, for the disease itself tends directly to induce a chloro-anæmic state.]

If a chancre be the portal through which the system is first tainted; if it be the "*continuing* source" of infection, as Dr. Humphry epitomises Virchow's doctrine; or even if it be a mark and effect of the general taint being already present, as many Continental writers, who have regard to the period of incubation which precedes its appearance, seem to think, its continuance must, nevertheless, be an abiding source of contagion to others, and, it may be, a focus of infection to the system, so that its early removal is not a matter of indifference; spite of the low appreciation in which the knife and caustics have been held by some high authorities (Dupuytren and Vidal). In practice we cannot determine, at early dates, what is the exact nature of any given ulcer. If it prove of the non-infecting type, then its progress is, without doubt, capable of being arrested by excision or destruction; if it be infecting, no harm is done. If a primary lesion can be excised thoroughly as well as early the practice seems to be good; but if no secondary symptoms follow, we can manifestly have no proof that the operation averted their occurrence. Mind, we do not say that excision or complete and early distinction of a *true* chancre

may not be regarded as very good practice in influencing the severity and duration of the consecutive phenomena, for it may, and must indeed, effect this if every primary manifestation be a present and future focus of contamination. But excision, at any rate, must have a very limited range for many obvious reasons—the site of the lesion, the presence of induration in the glands, and refusal on the part of the patient.

If cauterants be used, these must be of the strongest and most penetrating, such as fuming nitric acid or the stick of potassa c. calc. In passing, we may remark that a rapid and continued affusion of cold water wonderfully mitigates the pain after the application of nitric acid.

What are the signs in the primaries from which we may anticipate severe and protracted forms of constitutional disease? Of course we must not set aside the constitution, previous and present health of the patient, as indifferent elements in forming a prognosis, but we will confine ourselves to the features of the disease, and, here, an approximation to the truth is all we mean to aim at. The density, size, and extent of the induration, and its persistence, in the chancre and inguinal glands; the amount of ulceration in the chancre,—these appear to stand in some connection with the nature and severity of the subsequent phenomena.

Now, it must be conceded that, do what we will, secondary symptoms will almost invariably ensue after true chancre. Mercury does not *prevent*—save in the most rare and exceptional instances—the occurrence of secondary disease. Mercury, says M. Diday, has been obliged to abandon the pretension of so *radically* curing syphilis as to render all relapse impossible. Its claims to use in the primary stage are—that it dissipates induration, and heals ulceration quicker than any other remedy, and it has the power, at least, of delaying the appearance of syphilitic symptoms. Some excellent observers go further than this, we are aware; some—as, for example, Dr. Marston—believe that mercury does so modify, in some instances, the course of the disease that the ordinary external manifestations are slight, or do not appear, although the patient may exhibit indications of the syphilitic cachexia in his pallid aspect, impaired health, and liability to rheumatismal and other pains.

If, therefore, there be the history of a previous attack of syphilis, if the induration be moderate or small in amount, if the attendant ulceration be limited, if it do not prove very indolent, and can be healed by the application of local remedies, and the induration tends to disperse, and if, moreover, there be any *doubt at all* about the nature of the sore, mercury may be withheld. Wherever there is sloughing, rapid ulceration, &c., whether depending upon hereditary, constitutional, or other

causes, mercury is a dangerous remedy, and requires to be used, if at all, tentatively.

Now for the so-called secondary manifestations. It is here that mercury appears to us of such undeniable utility that most practical men, and patients too, are agreed upon its use.

So far we have endeavoured to hold the balances even, and we must here ask ourselves two questions: is the prolonged administration of mercury (such as Ricord advised, for instance) necessary or justifiable? Does mercury always effect a cure, and is it necessary in all cases?

To the first, we may say, that it is always better to try the effect of the endermic use of the mineral by the moist calomel vapour bath, or by inunction, before we administer it internally; and we must add that we do not believe that any such prolonged courses are needed or advisable. When mercury has been continued for a long period with a view of preventing relapse, the health may be much impaired, a cachectic aspect acquired, and a tendency to a diseased action in internal organs engendered. But it is no good contending for a position which has been long since surrendered, for no one gives mercury in the wholesale way which characterised Hunter's day. Acknowledging many of the evils alleged against mercury, our own observation has led us to form very different conclusions from those advanced by the anti or non-mercurialists. We can only regard the long array of charges laid to its door as highly exaggerated expressions. *If mercury be exhibited with ordinary care and discrimination, and particularly if it be applied through the skin, there would seem to be little or no risk of injury to the constitution.* Among the large number of cases which we have so treated, we have never witnessed any of those disastrous effects described by many authors. Of this we are quite sure, that some of the very worst forms of constitutional syphilis have appeared in those who have either taken no mercury or very little, and, on the other hand, the instances within our own cognisance are now numerous wherein the most remarkable recoveries from severe forms of constitutional syphilis have been effected by the aid of mercurials where every other treatment had been tried in vain. Those who are most severe in their denunciations of mercury—those who would seem to regard the use of the mineral with the greatest dislike, and who would debit every bad symptom which may appear in the course of a syphilis to the mercury used, instead of the syphilitic virus—seem to have lost sight of the fact that mercury alone has not only never been proved to be capable of giving rise to the combination of symptoms witnessed in constitutional syphilis when specifically treated, but that all experiment and observation is opposed to the assumption. If there be one thing more certain

than another, it is that mercury was not employed for the treatment of syphilis for many years after the epidemic in 1494-5. We possess a history of its dreadful ravages then, and the descriptions given leave no doubt whatever of the terrible scourge that it then was, and of the effects it produced upon the bodies of the sufferers and the minds of those who were eye-witnesses. Of a given number of cases of true syphilis not specifically treated, all will exhibit secondary symptoms of some kind, and of these a certain proportion will be of a severe and chronic type, and followed by the phenomena of the so-called tertiary syphilis. The proofs of these statements are too ample for us to do more than allude to them. Let it suffice that many of the patients which served to swell the Hamburgh lists went elsewhere to be *cured* of later manifestions; of those treated by syphilisation, some have exhibited tertiary phenomena, and very many more will turn up, in all probability; and there seems to be very good reason for doubting whether any remedy, or any combination of remedies, be as efficacious as mercury in preventing the hereditary transmission through the germ. M. Diday, of Lyons, has established, by direct observation, that out of forty-three patients in whom the natural evolution of syphilis was carefully watched, in seventeen the symptoms assumed such a serious aspect as to threaten permanent injury to the constitution, and with these he was compelled to have recourse to mercury. Diday is among the *most moderate* section of mercurialists, and his *positive* results may be relied upon. But it must not be forgotten that the *negative* observations can never be equally certain, because we know too well, unfortunately, that syphilis may remain latent for long periods, and manifest itself after very long intervals. In looking over our notes of cases, we find that of thirty-eight primary syphilitic sores not specifically treated twenty-one exhibited a mild form, eight a medium form, and nine a severe form of constitutional syphilis. How far these results might be modified if we possessed all the subsequent histories of these patients we cannot say, of course.

In order that our readers should more clearly perceive the tenor and spirit of our remarks on the therapeutics of syphilis, we must call their attention to the following illustrations of the forms, course, and progress of the disease.

We venture to think that a grouping into mild, medium, and severe types will be found to have some foundation in nature. We assume that the description applies to the first attack, because, as we have said already, there are either no subsequent ones, or these are very modified. The present and past health of the patients tolerably good: the subjects of no inherited taint.

1. *Mild.*—The chancre, as a rule, with but little induration, and affected with a slight degree of ulceration; the inguinal glands enlarged on one or both sides, and moderately so. Within from five weeks to three months, some enlargement of the cervical glands; vague muscular and articular pains; a slight febrile disturbance, perhaps, preceding the appearance of a dull-red, mottled roseola, with slight vascularity of the fauces, and very trifling loss of hair; subject to some manifestations of the disease, decreasing in intensity, at intervals, which become wider as the case progresses; iritis, if it appears, yielding easily to treatment; no (observed) tertiary manifestations.

2. *Medium Severity.*—Induration of chancre, and glands well marked and persistent; moderate degree of ulceration, which tends to become indolent and chronic; chloro-anæmia and osteocopic pains pronounced; exanthem (later in appearance than in the mild) papular, scaly; marked congestion of throat, which at later dates ulcerates; sores on lips and buccal mucous membrane; iritis, occasionally tertiary symptoms (nodes, swelling of tendons, &c., with pustular disease of skin). Frequent recurrences of syphilitic manifestations, which last longer and have shorter intervals of quiescence than the previous form. The disease produces very important modifications of the health, but is chiefly dangerous to life from a connection with predispositions to other constitutional maladies (hereditary or acquired).

3. *Severe Type.*—Induration large and chronic, in chancre and glands; ulceration well marked and phagedenic, sometimes extensive, and liable to return after healing; chloro-anæmia, loss of health, emaciation, loss of hair, well marked and gradually increasing, making up a cachexia; osteocopic pains severe; eruptions discrete, and affecting deeper layers of skin, or pustular and relatively later in their appearance; iritis sometimes double, and often chronic or recurrent; throat affection severe; tertiary symptoms very common, affecting scalp, bones, tendons, larynx, or subcutaneous tissues, and often accompanied by some symptoms referable to diseases of internal organs. If engrafted in one the subject of some hereditary predisposition the effects may be permanent, or the cause of death, from acting as the excitant to other morbid processes. Of course, it is not meant that these limits are arbitrary, for they are approximative only; nor is it intended that all the symptoms should be present in any given case, but that these will conform to one or other of the above types.

In the first variety mercury is not *required* at all for the primary and may be deferred to the secondary stages, and then only used when any of the symptoms become intractable or chronic, or in certain kinds of iritis, or when a scaly syphilide

appears. Warm baths, aqueous vapour baths, steel and diaphoretics, with strict attention to hygiene, often prove sufficient to cause a disappearance, at any rate, of the syphilitic manifestations.

In the second variety mercury alone appears to exercise any influence on the absorption of the interstitial deposits (indurations) present in the primaries, and frequently heals a chronic or indolent sore, when every other remedy has been tried; but mercury very rarely prevents the occurrence of the subsequent phenomena. For the cure of the constitutional manifestations, mercury, in the form of calomel vapour-bath, is by far the best remedy, and it is almost invariably followed by *most marked benefit*, if employed with ordinary care and judgment.

In the third variety the mineral is also the most trustworthy remedy for the dissipation of the primary symptoms, and it may be used, unless plainly contra-indicated by some constitutional causes, or a sloughy state of the chancre, particularly if the ulceration or unhealthy action in the sore advances when the mineral is employed. For the secondary phenomena it should be tentatively, and only endermically used with much discrimination, while the most diligent efforts are being made to raise the vigour of the body by tonics, good diet, &c. For the later stages mercury generally appears to be most injurious, but fortunately iodide of potass, in large doses, and iron here come to our aid as very reliable agents.

Dr. Wilks seems to us to have gone to the root of this question, in the paper to which we have already referred, when he speaks of true syphilis and its sequelæ. The *effects* of the virus are shown in the albuminous deposition, while the *sequelæ* exhibit the degeneration of the tissues from the long continuance of the morbid action. Now, mercury opposes the first, but favours the second, hence the opposite views entertained about this drug.—*Med. Times and Gazette, March* 18, 1865, *p.* 282.

80.—ON THE UNITY OF THE SYPHILITIC VIRUS.

By Geo. G. Gascoyen, Esq., Surgeon to the Lock Hospital, and Assistant-Surgeon to St. Mary's Hospital.

There is no difference between the pustule which gives rise to the indurated ulcer and that which results in the soft sore; nor between the early chancres themselves until induration has set in. The most strenuous advocate of the double virus theory has not attempted to diagnose the one sore from the other before this distinguishing feature has manifested itself; and we cannot be surprised, therefore, that grave mistakes have sometimes happened in experimental inoculation, where a very recent sore

is usually selected for the purpose, under the impression that its secretion is more energetic than from one of older date. If, then, it be true for syphilis, as it unquestionably is for vaccinia, that the *condition* of the fluid inoculated determines the nature of the disease, whether local or constitutional, an explanation is afforded of those hitherto perplexing cases where, for purposes of experiment, persons have submitted to inoculations with the the matter of soft chancres, considered by competent judges to be of the simple or non-infecting kind, and which, to the surprise and discomfiture of all concerned, terminated in ulcers which indurated and gave rise to secondary disease. It will account also for those instances—happily few—where indurated sores have arisen during the process of syphilization, when that horrible treatment has been practised upon persons for the cure of venereal sores, or for the *prevention* of constitutional disease.

There is no period of incubation for either the infecting or the simple ulcer, but a local action commences from the moment that the virus finds its way beneath the cuticle and comes in contact with the vascular tissues. Artificial inoculation therefore, by at once placing the matter under the skin, gives rise to a chancre more quickly than ordinary contagion, where some little time elapses before the fluid can pass through the epithelial layers which cover the dermis.

The early symptoms are the same whether for the infecting or non-infecting sore: a little redness appears at the point of insertion; then swelling becomes perceptible to the eye and touch; a papule succeeds, on the summit of which a vesicle speedily forms, increases in size, and is surrounded by a bright-red ring; the contents become turbid, then purulent, and the characteristic pustule is seen, beneath which is the chancre. The period required for the evolution of these phenomena varies; but in the case of the simple sore the pustule is usually formed on or before the fourth day, whilst it would appear that a little more time is necessary for the development of the infecting chancre. If, then, a pustule were somewhat tardy in its formation, I should be inclined to suspect that it would give rise to an ulcer of the infecting variety.

When the chancres are fully established differences begin to announce themselves. The ulcer, which will remain a local affection, becomes surrounded by inflammatory swelling, extending more or less beyond its circumference, and gradually subsiding in the adjacent tissues; its margins, vertical and sharply cut, are undermined and angry-looking; its floor, irregular and worm-eaten, has a greyish unhealthy aspect, and it secretes purulent matter copiously which can be re-inoculated again and again. The inguinal glands are not usually influenced by this,

but, when affected, it is a single gland which inflames and suppurates, the pus being sometimes of a virulent nature, capable of inoculation, and producing an ulcer similar to that which originated the bubo.

The chancre, which is the forerunner of constitutional infection, begins to lose its inflammatory margin somewhere about the eighth day, and to become circumscribed by an effusion of plastic lymph. This is manifest to the touch generally by the ninth or tenth day; it slowly increases, forming a complete bed for the ulcer, and extends deeply into the tissues around and beneath it. This induration has a peculiar cartilaginous feel and ceases abruptly, so that its outline is distinctly defined; there is no discoloration of the neighbouring skin, and, as the infiltration goes on, the edges of the sore, which are adherent to the parts beneath, assume a pale, shining appearance, showing the absence of all inflammatory action; the floor is smooth, red, and glazed, covered by a whitish, semi-membranous-looking secretion, which adheres closely, and is incapable of re-inoculation on the patient himself. When induration commences at the base of the ulcer the inguinal glands take on a similar action, becoming enlarged and hard, but painless, and continue in this condition for weeks, or even months; they rarely suppurate, and, if by chance they do so, the pus formed is never inoculable. The chancre has now assumed an indolent character, and the ulceration generally remains stationary for a considerable time, or else slowly increases *pari passu* with its bed of induration without any attempt at healing. This is not due, I believe, to any specific influence, but to a mere mechanical cause. The plastic effusion which constitutes the induration offers an obstruction to the free passage of blood to the part, so that there is not sufficient reparative power to heal the sore, or limit the ulceration; and now we see why local applications are incompetent to heal this chancre without constitutional remedies, and why mercury in such cases will often act as a charm.

This drug, by its depressing effect upon the system, appears to hinder the organization of plastic exudations, and is said to stimulate the lymphatics to increased activity, thereby promoting absorption of the indurated mass, and relieving the vessels from pressure, so that they can convey blood in sufficient quantity to enable the sore to heal. It is not until the induration has very sensibly diminished that cicatrization of the ulcer is obtained.

Another *questio vexata* now comes under consideration: How does the economy receive the virus, and why is it that the syphilitic poison should permanently contaminate and remain

in the system, whilst vaccinal disease, with which we have compared it, and whose early symptoms so closely resemble it, should give rise to such very different constitutional effects, as the only point of resemblance between these two affections, after they have ceased to be local, is the creation of a diathesis which prevents any further infection?

It has been maintained, that as syphilis becomes a blood-disease, it must enter the economy by means of the blood-vessels; but a little consideration will, I think, convince us that it is impossible it can gain admission in this way. When an irritant poison is introduced behind the skin determination of blood to the part immediately ensues, engorgement of the vessels and the various phenomena of inflammation follow, ending in suppuration. Constitutional disturbance is at the same time provoked, its severity depending upon the amount of local irritation. This is a healthy action on the part of nature to be relieved of the irritant, and occurs after a dissection-wound or the insertion of any foreign substance. When, however, the virus of certain diseases—and by virus I mean the active contagious principle, the essential element of reproduction of a disease—is introduced into the circulation, it excites not only local symptoms at the seat of puncture, but mingles with the blood, and speedily shows its effect upon the system by a general febrile movement, accompanied by certain manifestations—usually eruptive—which are peculiar to, and symptomatic of, each of these several affections.

These symptoms would appear to be an effort of nature to rid itself, by a constitutional revulsion, of the *materies morbi* which is present in the blood; and if successful in doing so, certain elements of the blood having been eliminated or altered or destroyed by the action of the virus, there no longer remains a nidus in which the germs of this particular disease can again find a resting-place, and the person remains free for ever from another attack.

If we except that which produces hydrophobia, we may say that all known animal poisons, when admitted into the circulation, produce their acute toxhæmic effects *at once*. If the syphilitic poison entered by the same channels, it should do so too; but we know this is not the case, as a considerable though varying period elapses before what are called its "secondary symptoms" make their appearance.

The poison of syphilis creates a local lesion, and gives rise to symptoms which are at first almost exactly similar to those of other inoculable diseases; but whereas, at the moment when contamination of the system occurs, the latter give evidence of their entrance into the circulation by exciting a general febrile

reaction, the former presents a marked contrast to them, by losing any inflammatory condition that may have attended it, and by the effusion of plastic lymph at the base of the sore, constituting what is termed "specific induration." Directly this symptom has set in about the chancre, the adjacent lymphatic glands become similarly involved, and give corroborative testimony of a general infection; the absorbent vessels, also, which lead to these glands may sometimes be felt as hard knotted cords, from a spread of the induration along their course. Does not this lead us to infer that the syphilitic virus, contrary to the other morbid poisons adverted to, enters the system *indirectly* through the medium of the lymphatics? The poison is carried by these vessels to the chain of glands in immediate relation with the sore, and is gradually passed on through the numerous sets of glands which successively intervene to prevent the admission of any noxious material into the circulation. During its progress the virus is subjected to the modifying agency of these organs, and by the time it has reached the venous system it is deprived of those active properties which an animal poison possesses when it is introduced *directly* into the blood; yet it has not altogether lost its power for harm, since it gives rise to a constitutional disease, which is usually ushered in with headache and fever, marking the moment when the virus has entered the general circulation. This vital effort is however slight, and totally insufficient to get rid of the poisonous matter, which contaminates the blood, traverses the body, vitiates its tissues, and remains in the system of the patient throughout life, rendering him at any time liable to an outbreak of the disease. A constitutional disorder is thus created which is capable of hereditary transmission, and, under certain circumstances, is even communicable to a healthy person by physiological contact.

This view of the manner in which syphilis infects the system receives strong confirmation from the fact which daily comes before us, that a person who is of a delicate or strumous habit of body—one in whom the glandular system is more especially at fault—suffers much more rapidly and severely from constitutional syphilis than is the case with one whose functions are unimpaired.—*Lancet, Dec.* 13, 1864, *p.* 737.

81.—ON SYPHILISATION.

By the Editor of the British Medical Journal.

Dr. D. J. Simpson in the 'Edinburgh Monthly Journal' gives a paper on syphilisation read by him before the Royal

Medical Society of Edinburgh. Dr. D. J. Simpson appears to be an out and out admirer of this new remedy. But he has, we conclude from his paper, no personal experience at all of its action. The following is a short abstract of his remarks. The announcement of Auzias-Turenne, that the syphilitic taint could be removed by syphilisation from the constitution has, during the last eleven years, been verified by the experiments of M. Sperino of Turin, and more especially by those of Professor Boeck of Christiania, to whose labours we are indebted for the greater part of our knowledge of this wonderful remedy. The following is an abstract of two cases of very severe constitutional syphilis which defied the ordinarily employed curative means, but which, under the care of Professor Boeck, yielded at once to syphilisation.

Case 1.—Mr. ——, contracted a chancre in 1853. After the lapse of a few months, the tonsils began to ulcerate. The ulcers, however, healed up under repeated applications of nitrate of silver. Iodide of potassium was then given internally, with sarsaparilla. No further symptoms manifested themselves until March 1858, when an ulcer formed on the inner side of the right knee, which healed up after being blistered and strapped. In December, 1859, the tonsils and soft palate became extensively ulcerated. No local application seemed to be of any avail in checking the ulceration, and the constitutional treatment by iodide of potassium and sarsaparilla proved equally powerless. In August 1861, the patient came to Edinburgh for advice, and was examined by several of the most eminent physicians and surgeons in that city. His tonsils and soft palate were almost entirely destroyed by ulceration. He looked quite cadaverous, and was so weak that he had the greatest difficulty in walking. His condition was the more alarming, as several members of his family had died of phthisis. He was advised to proceed to Christiania, and submit, as a last resource, to treatment by syphilisation. This he did; and arrived in Christiania on September 1st, 1861. On his arrival he put himself under the care of Professor Boeck, who commenced treatment at once by inoculating him on the chest with matter from an indurated chancre; reinoculating every third day with matter from the previous pustules. At the end of a fortnight the patient's appetite, which he had almost lost, became ravenous. He could also walk without experiencing much fatigue. Three weeks later, inoculation failed to produce any result; the successive crops of pustules having become gradually smaller, the last proving abortive. Inoculation was then tried on the arms; and pretty large pustules followed. After three weeks' treatment, the pustules aborted here also. The patient left, *completely cured*, on December 1st, 1861, having gained three stones in

weight, and expressing himself as "almost as well as he had ever been in his life."

Case 2.—Mr. —— was advised, in the summer of 1861, to go to Christiania and to place himself under the care of Professor Boeck, for a very severe attack of tertiary syphilis. Six years had elapsed since he contracted a chancre. In the summer of 1861, when he came to Edinburgh seeking advice, he had a large syphilitic ulcer over each tibia, and one over the left clavicle. Perforation of the palate had taken place, and some pieces of bone had come away from the interior of the nose. The frontal bone was swelled, indicating the commencement of a corona veneris. The ulcers were very obstinate, and continued to increase in size, in spite of treatment. The patient had previously undergone a complete course of mercury on two or three occasions. He had been treated by the heads of the profession in Edinburgh, London, and on the Continent of Europe, without experiencing the slightest benefit; and, despairing of recovery, he determined to go to Christiania. So reduced was he that he had to be carried on board the steamer on leaving Britain, and from it on his arrival in Norway. He underwent a course of syphilisation; and so rapid was his return to health and strength, that within two months after the time of his landing in Norway, he was able to hunt. After an absence of three months, he returned home perfectly cured. The cure in this case was the more remarkable, as the patient was labouring under an enormously enlarged liver, which he had acquired in China, when serving there as an officer in the army. Dr. Simpson saw both of the patients whose cases have been described, immediately after their return from Christiania. Both spoke in confident terms of the treatment. Up to the present time, neither of them has had the least sign of a relapse. Syphilisation may be practised either with matter from a *soft*, or with matter from an *indurated chancre*. Most late writers on the subject of syphilis maintain that the poison giving rise to an indurated chancre is essentially different from that which causes a soft chancre; but Professor Boeck has pointed out that both forms of chancre are the result of one and the same virus. According to him, it depends only on the difference of intensity of the virus whether a soft or indurated chancre be produced. Very intense virus gives rise to a *standard soft*, less intense, to a *standard indurated chancre*. Virus intermediate in intensity to these two extremes gives rise to intermediate forms, so that our prognosis must be always extremely guarded as to whether secondary symptoms will ultimately supervene. No satisfactory theory to account for the extraordinary disappearance of the constitutional symptoms under a course of syphilisation has yet been advanced. Dr. Danielssen of Bergen, has mooted

the idea that the action of the remedy is purely depuratory. In consequence of many persons holding this view, experiments were set on foot in Christiania to ascertain whether by means of irritants an equally happy result could be obtained as by syphilisation. Pustules produced by friction with tartar emetic ointment were the media employed. The committee appointed to investigate "Tartarisation" report as follows:—"The members of the committee all agree that they do not know any manner of treatment which works more efficaciously, or even so efficaciously, as syphilisation does in cases of secondary syphilis in individuals not previously treated by mercury." That the ulcers produced should be syphilitic, and syphilitic alone, seems to be the great secret of success, as is shown by Nature herself throwing off the disease by the production of tertiary syphilitic ulcers. In comparing the average of relapse after the different methods of treatment, we see that after mercury thirty-two per cent. relapse; after iodide of potassium, twenty-one per cent.; after syphilisation, only nine and a half per cent. Professor Boeck, however, since he has confined himself to inoculating with matter from indurated chancres, has not had to record a single relapse. The two modes of treatment which claim our chief attention, are syhilisation and tartarisation. After tartarisation twenty per cent. relapsed; after syphilisation only nine and a half per cent., or, according to Boeck's last series of experiments, not one. The greatly diminished chance of relapse, as well as the possibility of arriving at a certain point of immunity where we can pronounce our patient *cured*, which cannot occur with tartarisation—as in the latter case we can produce sores *ad infinitum*—shows the superiority of syphilisation over every method of treatment which has yet been proposed. At its first introduction, discredit was brought on the practice by its discoverer, Auzias-Turenne, proposing to employ the remedy as a prophylactic, like vaccination. This, of course, is revolting to the mind of every practitioner. Moreover, syphilisation does not act as a prophylactic, as one or two cases after having been cured by the treatment have subsequently contracted a fresh chancre, which has been followed by constitutional syphilis. Nor is its employment as a remedy for the primary sore in any degree a justifiable proceeding; for we can never tell whether any chancre will be for certain followed by constitutional syphilis. But as a remedy for the *constitutional* disease it stands pre-eminent for syphilisation appears capable of curing *every* case of constitutional syphilis. Surely, instead of being summarily rejected, as it has been in this country, the remedy is entitled to a fair and impartial trial.—*British Medical Journal*, *Jan.* 28, 1865, *p.* 89.

82.—COMMUNICABILITY OF SECONDARY SYPHILIS BY INTERCOURSE.

By W. Allingham, Esq., Surgeon to the Great Northern Hospital.

[The question "Can the mother be infected by the semen without having been pregnant?" is one usually answered in the negative.]

I have several times seen cases which did appear to point very decidedly to syphilisation of the female by the seminal fluid without pregnancy. Two of my cases especially made a great impression on my mind, and with your kind permission I will briefly relate them, because this is really a very important and practical question; and I think if every surgeon who has opportunities of carefully observing the varied forms of syphilitic disease would record his cases, we might at least hope to arrive at a more settled belief on many disputed points.

A gentleman, aged twenty-seven, very healthy, and residing in the country, came to me and showed me an indurated sore on his prepuce; it had existed four weeks. His wife had been confined one week ago, and he had not been able to have intercourse with her for two months prior to her confinement, as she had been in very delicate health. He could trace the sore to an impure connexion five weeks before my seeing him. Three weeks after our first consultation he manifested well-marked, but not severe, secondary symptoms. The sore was healed in four weeks, and all induration had disappeared at the termination of six weeks, while the rash was fully out upon him. He, having no sore throat, not the slightest symptom of the original sore, or any mucous tubercles, resumed intercourse with his wife, seven weeks having elapsed since her confinement. Between nine and ten weeks after the resumption of connexion, he brought his wife to me with decided secondary syphilis—squamous eruption, small scabs on scalp, sore throat and hoarseness of voice,—an examination with the speculum revealed ulceration of the cervix and copious uterine discharge of mucopurulent character.

It is now more than three years since this case occurred, and the child (which I ordered to be weaned directly I saw the mother) has not had any symptom of constitutional syphilis.

How did the mother become syphilised, if not through the seminal fluid of the male? I do not think that any deceit whatever was exercised towards me, and there was no reason why there should be. Both the wife and husband agreed that no intercourse had taken place for fully two months prior to her confinement; there was every reason to believe that she was a most virtuous woman, and there could have been no oppor-

tunity for her being otherwise; so, on the whole, I think it even more than probable that this was really a case where the seminal fluid of the male, without impregnating, induced constitutional syphilis. I would put it as a question, whether the uterus, seven weeks after parturition, was not more than usually susceptible of absorbing a poison?

Case 2.—An officer in the Peninsular and Oriental Company's service, after eight months' absence from England, arrived home two days subsequent to his wife's confinement. He was suffering from secondary syphilis, which he had been affected with some few months, and which had succeeded a primary sore contracted in Alexandria five months before he came home. For his secondary disease he consulted me. He had rather severe psoriasis, no sore-throat, no mucous tubercles, no sore, or remnant of one, on the penis. Between the fourth and fifth week after his wife's confinement he had intercourse with her, and three months afterwards he brought her to me suffering from severe secondary symptoms—ulcerated throat and rash. I was not able to make a vaginal examination. The child was weaned, and, when fifteen months old, presented no symptoms of syphilis. This, perhaps, is not so clear a history as the first case I have related, but still points most decidedly in the same direction.—*Medical Times and Gazette, Dec.* 10, 1864, *p.* 633.

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§3.—ON DIFFERENT MODES BY WHICH CONSTITUTIONAL SYPHILIS MAY BE COMMUNICATED.

By Henry Lee, Esq.

Until within a comparatively short period, the author observed, it was of late years a well-nigh universally received opinion in Europe, that syphilis could only be communicated by a primary syphilitic sore; that a chancre which was followed by secondary symptoms was always the result of contact with the secretion of a similar affection. Great and grievous have been the injuries inflicted by this theory both on single and on married persons. For anyone who in any way had contracted syphilis under the reign of this cruel dogma, was pointed at with the finger of scorn, was accused of having necessarily contracted the disease in some illicit way, and was not unfrequently most unjustly excluded from society. More than this: Cases not unfrequently occurred, and have occurred under the author's own observation, where legal proceedings have been threatened or actually carried into execution, in consequence of a person having, as was supposed, contracted fresh disease after marriage, the parties instituting the proceedings being misled by the doctrine which then prevailed. The following case was given in illustration:—A gentleman who had had syphilis married.

A few months afterwards his wife was found to have some indurated tubercles about the labia, with corresponding enlargement of the inguinal glands. A secondary eruption followed. The friends of the lady ascertained the nature of the disease, and being instructed according to the prevailing theory, accused the husband of having been unfaithful after his marriage. In this stage of the case the question was put to the author, "Can the husband have given the disease to his wife without himself having had a primary chancre?" Mr. Lee was enabled here entirely to clear the husband's character as far as his conduct subsequent to his marriage was concerned.

Nor is the evil which this false theory has produced restricted to private life. Many and various have been the plans suggested for the prevention of the extension of disease amongst our soldiers and sailors; but while much pains have been at times bestowed to prevent the contagion of primary disease, comparatively little attention has been given to the subject of the equally contagious nature of certain secondary affections. It is to be hoped that the Government Commission appointed to investigate this subject will now take into consideration the whole of the facts calculated to illustrate the real nature of the disease, the best mode of preventing its extension, and the most approved methods of treatment.

Scarcely six years have elapsed since a most eloquent and able course of lectures was delivered, the principal aim and object of which was to prove that the whole series of secondary syphilitic affections were not inoculable, and most carefully and fully did the talented author of those lectures express the opinions which were then generally received. Since that time the subject had not, Mr. Lee believed, been brought fully under the notice of the Medical Society, and he had ventured, therefore, to do so on the present occasion.

Cases were then read illustrating the communication of the disease to a healthy woman from a diseased child by means of an indurated ulcer on the breast, and the communication of the disease by vaccino-syphilitic inoculation.

Numerous other cases of vaccino-syphilitic inoculation have now been recorded; and Dr. Viennois quite recently communicated to the Medical Congress at Lyons two fresh observations in addition to those which had previously been collected. On the 15th of May, 1862, six children were vaccinated. Five out of the six between three and four weeks afterwards had indurated ulcers on the inoculated parts. Each one of these five children communicated the disease to members of its own family, so that twenty-three persons were infected from this single source. The mothers and nurses of these children had first a chancre on the breast. The fathers of the children and the fathers of the

nurses remained healthy until some form of secondary disease appeared on the private parts of the mothers and nurses, and then the husbands in some cases contracted the disease, which appeared in the form of a primary chancre. In a second case mentioned by Dr. Viennois, two children were vaccinated from the same source, and at the expiration of thirty-five days they both had an indurated chancre upon the inoculated parts.

Dr. Viennois had proposed, in order to prevent the recurrence of such serious mischief, that the inoculation of cow-pox or the horse-pox from the animals themselves should be substituted for the ordinary mode of vaccination; and it appeared that at Naples there actually exists an establishment for the purpose of carrying out this idea. The cow-pox is there inoculated on heifers. These heifers are driven about to different parts of the town as donkeys are here for the purpose of furnishing donkeys' milk. When anyone wishes his children to be vaccinated, a heifer is brought to the door: the driver removes a vesicle with a pair of scissors and gives it to the doctor on a pair of forceps, who then vaccinates the children with its contents. The fee for this kind of vaccination is five francs. The author observed that in speaking of vaccination our foreign neighbours very frequently use the term "vaccine pustule," and he had reason to believe that want of accuracy here might lead to some very serious mistakes. The vaccine poison produced a disease which essentially was not pustular. The vaccine lymph, examined under the microscope, contained no pus-globules at the time when it was fit for use. If the vesicle were allowed to remain, no doubt pus-globules would be formed; and if the purulent fluid were inoculated, a suppurating sore might be produced instead of the proper vaccine vesicle. Attention to the nature of the inoculated fluid he (Mr. Lee) believed to be very important, and thought that many of the "bad arms" seen after vaccination might depend upon other matters besides the pure lymph having been inoculated. There could be no doubt, he remarked that the action which produced a pustule was one of an entirely different nature to that which produced a vesicle; and unless these two actions were practically distinguished, medical men ran a great risk of inoculating a different disease to that which was intended. In these observations he did not allude to the presence of any separate specific poison in the inoculated matter, but simply to the kind of action which was intended to be communicated by the operation. If pus were inoculated, a pustule would probably result; it would be a suppurative action, and liable to all the complications which occasionally follow suppuration in a part. The author was informed by Mr. Tomkins, of the National Vaccine Establishment, that he had seen very serious constitutional effects follow the inocu-

lation of the cow-pox taken from the cow. How far such effects depend upon pus being inoculated instead of lymph demanded our serious consideration. In the interest of science, we were called upon carefully to distinguish these two actions, and in practice certainly vaccination should never be performed with matter taken from a pustule.

In the year 1854 the author published some lectures on syphilis which had been delivered the previous year at the Lock Hospital; and, in the fourth lecture, he dwelt upon the two kinds of results produced by syphilitic inoculation. He then stated: "In one case the appearances are in their origin—those of the adhesive inflammation; in the other, of the suppurative inflammation. And these constitute two very different classes of disease, both in respect to the local affections and their constitutional consequences." At this time every syphilitic inoculation was generally supposed to produce a "pustule"; and the production of this pustule was not only taken as the sure evidence of the syphilitic nature of the disease, but as a test for the necessity of giving mercury for its cure. Had we earlier learnt to distinguish the different kinds of morbid action which syphilitic inoculation gives rise to, much of the confusion which has reigned with regard to this branch of knowledge would have been avoided. It is now generally admitted that real syphilitic inoculation does not produce a pustule; that the disease which infects a patient's constitution commences as an abrasion, a pimple, or a tubercle; that the disease which does produce a pustule when inoculated is a disease of an entirely different kind, and that its effects are entirely local. Those who, therefore, maintained that the production of a pustule by inoculation was an indication for the administration of mercury, were advocating exactly the reverse of that which more recent observation has established.

There can be no doubt but that the real infecting form of syphilis, commencing with a form of adhesive action, will, during some period of the existence of the primary affection, give rise to a puriform secretion, just as the vaccine disease will, after the ninth or tenth day, produce some pus-globules; but that does not in any way prevent us from recognising the essential characters of the disease, in its origin and in the induration which is subsequently produced, as quite distinct in one case as in the other from any purely suppurative action. Mr. Lee was led to dwell somewhat upon this subject as the distinction which he drew between the two kinds of syphilitic sores as far back as 1854 has been called in question by some of our continental brethren. They have said that a true chancre will secrete pus; a fact which Mr. Lee had always allowed. And so, he observed, will a vaccine vesicle, especially if irritated.

In one case as in the other the disease begins without suppuration; and, if accidental causes of irritation be prevented, a very few days will leave the disease marked by the circular induration, and unaccompanied by any suppurative action whatever. One of the author's critics had supposed that in some drawings published last year of the microscopical appearances of the secretions of the two different kinds of syphilitic sores Mr. Lee had fallen into a mistake from not knowing the appearances of a pus-cell under the microscope. Mr. Lee thought he might at least have escaped this criticism had he mentioned that his friend, Prof. Beale, had been good enough to make the drawings in question for him.

The two kinds of action which were now described were very important, as enabling us to understand some forms of inoculation which otherwise would appear to follow no fixed law; particularly cases in which there has been a twofold inoculation, and where a mixed form of chancre has resulted. Such cases are not unfrequently observed in practice, but the evidence of their twofold origin is there not so satisfactory as if it had been the result of experiment. The evidence from direct inoculation is not, however, wanting. M. Melchior Robert has shown that if the secretions from the two kinds of chancres be inoculated by a single puncture on a healthy person, a soft chancre originating in a pustule will first appear, and subsequently an indurated chancre followed by secondary symptoms. M. Lindwurm has proved that if the secretion from an indurated sore be inoculated on a soft or suppurating sore, that sore will gradually be transformed into an indurated chancre, which will be followed by secondary symptoms; and M. Rollet has often demonstrated the fact that if the pus from a single chancre be placed on the surface of an indurated sore, the affection will assume all the characters of the "mixed chancre."

The limits of the present communication would not allow the author to go more fully into the subjects of the different kinds of secondary affections which are communicable by inoculation, nor of the inoculability of different kinds of secretion, as the milk, and the semen derived from a syphilitic subject. He would only remark, that, so far as his own knowledge went, he was not prepared to admit the inoculability of any secretion derived from a syphilitic subject without some increased or diseased action in the organ producing that secretion. Thus, the saliva from a syphilitic person with a healthy mouth would not, so far as he knew, produce syphilis in another person. The milk from a healthy breast of a syphilitic woman would not produce the disease in the mouth of a child. The semen from a syphilitic person with healthy organs of generation would not produce syphilis by being brought into contact with the mucous

membrane of the vagina. But let any of the parts become diseased, or let them be subject to increased action in a syphilitic person, and then the secretions from those organs have a power which they had not before, and might communicate the disease direct to any part with which they came in contact. The following case was given in illustration :—

A gentleman requested to know what was the matter with him, and was informed that he had a chancre which would infect his constitution. He said it was impossible, as he had never seen but one person, whom he had known for a long time, who had nothing the matter with her, and who was quite willing to be examined, if the fact were doubted. This person was accordingly examined. The mucous membrane of the vagina was red and congested, and a muco-purulent discharge issued from the vagina. There was no sore or abrasion of any part. On examining the chest and arms a distinct syphilitic eruption was visible, which she said she had had for some years, and believed to be the "nettle rash." It was then apparent in this case that the man had not been infected by this person, although in the way of becoming so, until, from some cause or other, some local increased or diseased action was produced in the organs of generation, and that then the secretion from those parts was inoculable. In this case the patient obtained the certificates of two medical men to say that she was free from disease.—*Lancet, Dec.* 10, 1864, *p.* 661.

84.—ON THE DIAGNOSIS BETWEEN SYPHILITIC AND NON-SYPHILITIC DISEASES OF THE SKIN.

(An abstract of a paper read before the Harveian Society by Dr. B. Squire.)

The author, after remarking on the importance of the subject, observed that an eruption, even when the question of antecedent primary syphilis was well made out, was by no means necessarily of syphilitic origin; and that where an eruption was clearly syphilitic, it was often extremely difficult to obtain a history of primary syphilis. He, therefore, placed very little reliance on inquiries of this kind as a means of diagnosis. The best evidence that we could obtain of the nature of an eruption was the evidence that was within our personal knowledge. The most reliable direct evidence of previous chancre was the cicatrix left by that chancre. One of the most important means of distinction between syphilitic and non-syphilitic eruptions was the situation of the eruption. The most frequent situations for syphilitic eruptions in the order of their frequency were the neighbourhood of the alæ of the nose, the angles of the mouth, the forehead near the roots of the hairs,

the back of the neck near the roots of the hairs, the inner canthus of the eyelids, the middle of the chest, the inner surfaces of the limbs, the neighbourhood of the axillæ and groins. It might be stated generally that the face was the most favourite locality for syphilitic eruptions. The age at which syphilides most commonly appear for the first time was another means of diagnosis. The most common age for this was between twenty-five and thirty-five. Infantile syphilis appeared generally at the age of one month. The earliest time at which the author had met with a syphilitic eruption dependant on syphilis communicated by intercourse was in the case of a female aged eight years. The colour of the eruption was a favourite, but, unless under certain restrictions, a fallacious means of diagnosis. He meant that the notion that a coppery tint would enable one to distinguish syphilitic from non-syphilitic eruptions was an erroneous one : this tint was found more or less marked in every case of non-syphilitic psoriasis. Again syphilitic eruptions did not present it at all at their outset ; it was only as they became developed that the coppery tint gradually appeared. The form of the eruption was a more reliable means of diagnosis. Most commonly this was annular or oval. If not in actual rings, the eruption formed segments of circles—incomplete rings ; or by the juxtaposition of two rings, it might form figures of eight. This, of course, however, would not enable us to distinguish, as by a touchstone, syphilitic from non-syphilitic eruptions. Psoriasis, *e.g.*, sometimes occurred in rings ; so did lichen ; so again did herpes circinatus. Lupus was another example of a non-syphilitic eruption of annular form. The absence of itching and smarting was a peculiarity which would often aid the recognition of a syphilide. The mixed character of syphilitic eruptions was another means of distinguishing them. Thus, in an eruption that was not syphilitic, it was rare to find rashes mixed with vesicles, or pustules with scales. Not so in a syphilide. What might be termed the "products of the eruption"—such as the scales or the crusts —were a means of diagnosis. The scales of a syphilitic eruption were finer, smaller, and more adherent to the surface beneath than those of a simple squamous disease. The scaly patch, too, was circumscribed by a whitish border, to which great importance had been attached by Biett as a diagnostic symptom. This whitish border was the result of the separation of the epidermis from the cutis around the diseased patch. The crusts, which might either follow the scales or succeed to pustules, were much harder, thicker, and greener, than the crusts of a simple cutaneous disase ; they were sometimes, at all events at their centre, as thick as they were broad. The ulcers that were left by either pustular or tubercular syphilides were generally cir-

cular, with abrupt and perpendicular edges, and a grey pultaceous floor, the skin around them having a copper tint. The scars left by a syphilide, though at first of a violet hue, soon acquired even a more tawny-brown tint than the eruption that had preceeded them ; this tint in its turn at length faded, but even when the scar had at length been left perfectly white, its annular or recticulated form, as the case might be, would distinguish its origin. Another means of recognising a syphilitic eruption was attention to other changes produced by syphilis besides those wrought in the skin itself. With regard to the sore-throat, we should be on our guard against relying too much on it as a diagnostic symptom. There were many diseases of the skin (and some that resembled pretty closely one or other of the syphilides) that were dependent on scrofula, or, at all events, occurred more frequently in the scrofulous than in any other constitution. No people were more liable to sore-throats than the scrofulous, and perhaps no two diseases were more frequently confounded together than syphilitic lupus and the lupus that occurs in the scrofulous. In doubtful cases the question, "Have you suffered from sore-throat?" is often allowed to settle the difficulty. The most characteristic condition of a syphilitic sore-throat was either the presence of mucous tubercles or of cicatrices. The presence of mucous tubercles, either in the throat or elsewhere, was one of the most conclusive signs of syphilis that one could have. The erythema of the buttocks and scrotum, that was so common in sickly children whose cleanliness was hardly attended to, was often mistaken for infantile syphilis, and if they happened to have a cold at the same time, it was still more likely to be set down to a specific cause. This mistake happened the more commonly that simple erythema in infants, as it faded, often assumed more or less of a tawny tint. The effect of the common remedy for infantile syphilis—grey powder—on the other class of cases, was greatly to aggravate them. Again, the author had frequently seen severe cases of eczema in infants taken for infantile syphilis, and treated for it with the same unfavourable result. There was no better or surer means for establishing the disease to be syphilitic than the existence of mucous tubercles when these were present, and in the majority of cases of infantile syphilis they were. Their most common situations were, the angles of the mouth, the alæ of the nose, the vulva, the anus, the scrotum, or within the mouth. The author then referred to rheumatic pains in the limbs, along the bones, or in the larger joints,—to bitemporal neuralgia, to falling off of the hair, to indolent swellings of the glands of the neck, and, in the female, frequent abortion, or still-births, or suspension of the catamenia (not arising from any other ascertainable cause), as

evidences of the syphilitic nature of an eruption, and to the chronic course and the progressive changes in the character of the syphilides, observing, however, that it was not upon any one symptom alone, but on the general issue of the evidence afforded by many symptoms that we should rely for our diagnosis of syphilides from the other diseases of the skin. He concluded by expressing his regret that the limits of a paper precluded his entering on the much larger subject of the diagnosis of the different syphilides from the several diseases of the skin for which they might be taken.—*Medical Times and Gazette, Jan.* 28, 1865, *p.* 103.

85.—CHANCRE AND SYPHILITIC LICHEN ATTENDED WITH INTENSE PRURITUS.

(Case under the care of Dr. Sisson, at the Royal General Dispensary.)

In September a German, aged twenty-six, presented himself, and stated that ten days or a fortnight ago he observed a small pimple on the glans penis, which, from the intense itching accompanying it, he was compelled to scratch. This he had done constantly to the present time, and he was now afraid that the penis was mortifying.

On examination, a large indurated chancre was discovered on the glans penis, covered with a black scab; the parts around were much inflamed, but there was *no bubo*. Ordered a lotion of lead and opium, which was changed in a week for one of sulphate of copper, and a mixture containing bichloride of mercury and iodide of potassium thrice a day. In three weeks the chancre healed.

On the 7th December the patient again presented himself, suffering from sore-throat, enlarged post-cervical glands, and syphilitic lichen on the face, chest, and back. He stated that the eruption itched so violently when in bed that he was unable to get any rest. Ordered one drachm of the solution of bichloride of mercury with five grains of iodide of potassium in camphor mixture, thrice a day.

Dec. 17th. Sore-throat well; eruption on back and chest has disappeared entirely, that on the face nearly. Itching had entirely ceased after taking the mixture three days.

Dr. Sisson has always subscribed to the opinion that the absence of itching is one of the most valuable diagnostic signs in syphilis, and this case only proves that there is no rule without an exception. The case, he says, is also valuable as showing the fallacy of Ricord's dogma, that there can be no infecting chancre without an indurated bubo. The absence of eruption

on the limbs and its presence on the face were strong presumptive evidence that the lichen was not intermingled with scabies, and the result of the treatment has, he thinks, clinched the matter.—*Lancet, Dec.* 31, 1864, *p.* 743.

86.—MERCURIALISED COLLODION AS AN APPLICATION TO CONDYLOMATA.

A patient aged fifty-six, under the care of Dr. G. Finco of Padua, had around the anus a large number of condylomata, which had increased in size under the use of nitrate of silver. Dr. Finco mixed twenty-five *centigrammes* of corrosive sublimate with fifty-two *grammes* of collodion, and, having shaken the mixture well, applied it with a brush over the two largest tumours. The next day, these had almost disappeared. In the course of sixteen days, Dr. Finco destroyed more than sixty condylomata; and the patient had no return of his disease.—*British Medical Journal, Jan.* 14, 1865, *p.* 44.

87.—ON THE NON-MERCURIAL TREATMENT OF SYPHILIS.

(Cases under the care of R. W. Dunn, Esq., at the Farringdon Dispensary.)

Case 1.—M. W., aged two months, came under notice on Aug. 1st. The mother of this child had suffered from sore-throat and eruption of the skin, and had been in-patient at one of the London hospitals for some months on account of it. The infant first showed the sign of an eruption at four weeks. When first seen, it had syphilitic psoriasis on the buttocks, snuffled very much, and was gradually wasting. Two grains of chlorate of potash in water were ordered to be taken three times a day.

Aug. 28th. The mother brought the child, fearing it would be suffocated. It could scarcely breathe through the nose, which was ordered to be well syringed with warm water, and the medicine continued.

Sept. 22nd. The infant had taken the medicine up to the present time, and was discharged cured.

Case 2.—C. M., aged three months. The mother of this child had suffered from constitutional syphilis, and had had three miscarriages before this infant was born. The child had been ill for five weeks previous to its coming under Mr. Dunn's care, and upon examination was found to be very debilitated and much wasted, with a syphilitic eruption on the legs and arms, mucous ulcers on the lips, and snuffles. The same medicine was ordered as in the previous case. The little patient came under treatment on the 11th of August, and was discharged cured on the 22nd of September.

Case 3.—N. W., aged two months. This child's mother had suffered from ulcerated sore-throat more or less for a long time, but there was no distinct history of syphilis. The child had a syphilitic eruption on the face, body, arms, buttock, snuffled very much, and did not take the breast so well as usual. Ordered two grains of chlorate of potash three times a-day. This treatment was commenced on Aug. 15th, and continued until Sept. 20th, when the patient was discharged cured.

Oct. 20th. A relapse having occurred, the same medicine was again ordered to be given.

Nov. 3rd. Discharged cured.

Nov. 10th. Had an attack of measles. No relapse since.

Case 4.—G. G., aged four months. Both parents syphilitic; the mother had four living children previous to this one, and frequent miscarriages. All the former children died before six months, and all had eruptions, which had been treated with grey powder and mercurial ointment. Upon examination it was found that this child had syphilitic eruptions on the buttocks, head, arms, and legs; ulcer on the anus; snuffles; is very thin and aged in appearance. Ordered it two grains of chlorate of potash three times a day. The medicine was continued till Nov. 24th, when the patient was discharged cured.

Case 5.—H. F., aged thirteen weeks. No history; a nurse child. Had syphilitic eruptions on the buttocks, ulcers on the anus, and mucous ulcers on the lips; rejects food; very emaciated; diarrhœa, extremities cold.

Oct. 6th. Ordered it injections of strong beef-tea by the anus—a tablespoonful about every four hours,—and a drop of the tincture of opium by mouth in a little chalk mixture every six hours.

8th. Diarrhœa and sickness stopped. Ordered the child to be fed upon milk and water and a little beef-tea, and to take two grains of chlorate of potash three times a day.

14th. Much better, and continued improving. Discharged cured Nov. 24th.

Case 6.—L. J., aged six months. Was quite well till vaccinated; four weeks after an eruption appeared on the back, legs, and face; snuffles. No history of syphilis. Upon examination this child was found to be very much emaciated, with an eruption on her back, legs, and arms; ulcer on the anus; snuffles. The infant's face had that peculiar aged look which is so often seen in syphilitic children. Ordered three grains of chlorate of potash three times a-day on Nov. 3rd.

Continued the medicine till Dec. 1st, when it was discharged cured.

Mr. Dunn has related these six cases from amongst thirty

of a like character, which he had treated in a similar manner, with only one death.

Statistics as to age when the first appearance of the symptoms had been noted.

				Cases.
Before the completion of	one	month	after birth ...	4
,,	,,	two months	,, ...	9
,,	,,	three months	,, ...	7
,,	,,	four months	,, ...	1
,,	,,	five months	,, ...	1
,,	,,	six months	,, ...	1
,,	,,	seven months	,, ...	1

—*Lancet, Dec.* 31, 1864, *p.* 742.

88.—ON TWO NEW SPECIFIC REMEDIES FOR GONORRHŒA.

By Dr. Thos. B. Henderson, Glasgow.

[The following paper was read before the Glasgow Medical Society. The paucity of remedies for gonorrhœa has been regretted by almost every general practitioner.]

The first of the medicines I have to introduce to your notice is the Oil of Yellow Sandal wood. It is obtained by distillation from the wood of the tree Sirium myrtifolium, of the genus *Santalum*. It grows in the East Indies. One lb. of the wood yields 2 drams of the oil. Lindley, writes, "This oil is said to be used to adulterate the oil of roses." Professor Redwood in his Supplement to the Pharmacopœia, on the authority of Dr. O'Shaughnessy, writes, "Sandal wood in powder is given by the native physicians in ardent remitting fevers. With milk it is also prescribed in gonorrhœa." I have ransacked many works on Materia Medica, and have not found even the name of Yellow Sandal wood.

In my experiments with this drug I have found it perfectly innocuous even in large doses. From 20 to 40 minims three times a-day, diluted with three parts of rectified spirit, and flavoured with ol. cassia or ol. cinnam., is the ordinary formula I employ; water and a confection after. In cases of the disease at the first, second, or third stage, in susceptible persons, I have often seen the most marked suppression of the discharge within forty-eight hours. It has the great advantage of being a pleasant medicine, not liable to cause sickness, agreeable to the taste, and grateful to the stomach. It is a medicine as to efficacy, in my opinion, equal, and frequently superior, to bals. copiab. or cubeb pepper. I have often succeeded with it, when both had been fairly tried, and failed. Besides, it is convenient and portable; and if the patient is delicate, or in bad health, or the

system disordered, the possession of a remedy which will act as a stomachic medicine, and cure the disease, is, I think, to be highly valued. I have used it in many cases during the past five years. I have no theory to offer as to its mode of acting. My experiments have been numerous, but entirely of a practical character. The odour of the drug is slightly perceptible in the urine. Its action on the urethra is observed, in susceptible cases, within a few days after beginning its use. Almost every druggist keeps it for perfumery purposes.

The other remedy I have experimented on, is the Gurjun or Gurgina balsam, or wood oil. It is the product of the *Dipterocarpus turbinatus*, an immense tree growing in different parts of India. Incisions are made and heat by fire is applied to the root. One tree yields about 40 gallons in a season, distilled with water, it yields 35 per cent. of volatile oil. Wood oil is a liquid of the consistence of olive oil, of a dark reddish colour and slight odour. Pereira gives a good account of this medicine when speaking of the adulterations of bals. copaib. In the new edition of Royle's Materia Medica, p. 319, it has the honour of occupying one line. Referring to the products of the Dipterocarpæ, it is written, "There is a wood oil which contains a principle analogous to copaiba." In the other works of Materia Medica it is either not mentioned or only slightly noticed. The description of this medicine which caused me to try it for myself, is contained in the valuable "Manual of Practical Therapeutics," by Edward John Waring, of the East India Company's Service. Smith, Elder, & Co., Cornhill, 1854, pp. 200. From which I beg to make the following extract.

"Gurjun or Wood oil tree is found at Chitagong, Pegu, the Tenasserim Provinces, &c. It is found abundantly in all the bazaars of India. By distillation it yields an essential oil, which in all its medicinal properties and actions closely resembles copaiba. Dr. O'Shaughnessy employed it in numerous cases of gonorrhœa and gleet; and the results seem perfectly conclusive, that in the treatment of these and other affections of the genito-urinary system, the essential oil of gurjun is nearly equal in efficacy to copaiba. It generally causes a sensation of warmth in the epigastrium, eructations, and sometimes slight purging. It greatly increases the quantity of the urine, which has a terebinthinate odour. Dr. O'S. found that some obstinate cases of gonorrhœa and gleet, which had long resisted copaiba and cubebs, were cured by this remedy. E. J. Waring writes, 'In the few cases I have had an opportunity of trying it, the results have been uniformly satisfactory. It might be advantageously introduced into English practice as a cheap and efficient substitute for copaiba. The dose is 10 to 15 drops, thrice daily.'"

It is now several years since I commenced to experiment with wood oil. I have only used it in cases where copaiba had been fully tried and failed. In every case it was successful within a week. No symptoms of inconvenience in any of the cases were produced. I gave it in what may be called large doses, a teaspoonful two or three times a-day, *uncombined.* I have not been able to investigate its action further as my supply became exhausted, and it is not easily procured in this country. I am thoroughly convinced it is an excellent medicine. I think it is probable this oil was introduced into England without there being a demand for it, and those holding it tried to get quit of it by mixing it with copaiba. Being detected, the cry has gone against its use in that way, and very properly so, fulfilling the old saying, "Give a dog a bad name, &c." That it can be procured abundantly at a moderate price, I have no doubt.—*Glasgow Medical Journal, April* 1865, *p.* 71.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

89.—ON SOME OF THE SYMPTOMS OF EARLY PREGNANCY.

By Dr. C. H. F. Routh, Physician to the Samaritan Free Hospital for Women and Children.

[The symptoms upon which reliance is usually placed in forming a diagnosis are well known, but Dr. Routh in the following paper introduces what may be called an additional symptom, which can in most instances be detected.]

Hitherto much reliance has not been placed on *auscultation* in the first period of pregnancy. The so-called placental souffle, if heard at all, has been heard quite at the end of this period. Dr. E. Kennedy of Dublin, it is true, has given some examples in which he was able to hear it with certainty at the twelfth, eleventh, and even in one instance at the tenth week; but his experience has not been confirmed by others. Nägele states that it is only heard after the fourth month. Rigby admits that it can be detected as early as the fifteenth or sixteenth week. Murphy states that it is not generally heard before the fourth month.

Years ago, Dr. Rigby premised that there was every reason to suppose that it might be heard at a still earlier period, if the uterus were at this time within reach of the stethoscope. The object of this communication is to show the means by which I have attained to this end; and henceforward, I imagine, it will be in the power of all occasionally to foretell pregnancy by this symptom as early as from the seventh week to the ninth, probably even from the sixth, when the placenta is situated near the cervix. At any rate, this is the earliest period at which I have heard the distinct souffle. At this early date, however, the sound differs somewhat from the placental souffle, as heard at a later period. Nor is it very easy to give an accurate description of it in words. It is more like the sudden cessation for a moment of a general splenic murmur. If I may so express it, it is a sudden interruption of an intensely vesicular and muffled murmur, synchronous with the pulse. This it is which makes it very difficult to catch sometimes, and which requires the closest attention and a most practised ear to make

it out at all. I have also observed that the souffle, when it does occur at a very early period, and when very distinct, has somewhat a high pitch; so far confirming the opinions enunciated by authors, who have described it as assuming a piping character when the placenta is attached near the cervix. Curiously enough, in those cases where I have heard this high toned pitched murmur *per vaginam*, I have heard it very early also in the iliac region of one side.

It is but right, however, to add that, as with ordinary auscultation-sounds heard over the abdomen, so with these pelvic sounds, they may be absent altogether. Sometimes we may lose them at a sitting, and are not again able to find them. Thus much, however, I think we may safely conclude, that during the first period, and in a doubtful case, no examination is complete without the use of the vaginoscope.

The cases now related amount to nine, in which the pregnancy was detected as early as from the sixth to the thirteenth week. All but one were married, and therefore there was no object in deceiving me; and, as I saw them all purposely, and by special appointment, to make out whether pregnancy existed or not, no difficulties were put in my way in the examinations made.

Of course, these do not include all the cases which I have examined or noted particularly from the sixth to the thirteenth week; but they are fair samples among several others, a few of which I regret I have been obliged to set aside, as I omitted to note at the time the date of their first examination by me. In these also, however, the pregnancy was made out very early. I have named these several instruments which I now show you *vaginoscopes*. Perhaps, were I to adopt the plan followed in the words *stethoscope* and *ophthalmoscope*, it would be better to call them *coleoscopes*, using two Greek words, instead of a compound of Latin and Greek. The examination of the uterus was made with these instruments introduced within the pelvis in other ways. Thus, a flexible tube passed up *per anum*, with an earpiece at the distal end, answered for auscultation. This method, though, as it might be supposed, useful, was sufficiently disgusting to discourage any further attempts. I have also attempted to hear through the sacrum; but the indications here were very uncertain. Stethoscoping *per vaginam* is not new. I am told that the late Dr. Stroud had devised a plan to effect this, and, if I mistake not, some German author; but I have not yet been able to obtain their memoirs. An examination *per vaginam* does not, however, in these days entail the same reprehension as formerly. Thus my facilities have been greater.

If a woman be placed on her side, with her legs very much

flexed upon her abdomen, then auscultation may be readily made with a common stethoscope, provided the distal end be small, by pressing directly with it against the perineum. This, by the apposition of its internal surface, impinges against the vagina and uterus, and so we can often hear sounds originating in the latter organ. Where, however, the woman is fat, and the perineum thick, the sounds are muffled. In this case, it may be passed *per vaginam*, and made directly to impinge upon the uterus. With a glass syringe, in doing this, as the rubbing of the clothes is not heard, no exposure whatever is needed. The same auscultation may be made with the woman on her back, with a curved glass speculum such as I show you.

It would be quite a little history, were I to enumerate the several plans I have adopted in the hope of devising a more perfect instrument for the purpose. I have tried metal stethoscopes, bent at a right angle, sometimes using a thin membrane at the vaginal end, of India rubber. I have used metal tubes, with a membranous sac in the interior, so constructed that I might blow air into it at will, and make the sac more or less tense. I have used wooden vaginoscopes, with a membrane interposed, at the union of the vaginal with the external portion of the stethoscope, which was here enlarged so as to form a cavity of greater dimensions than on any other part of the stethoscope. This cavity I have made exactly elliptical, so that the sounds conveyed would be concentrated at a focus, and be carried thence by a tube to the ear, upon the principle of Mr. John Marshall's ear-trumpet. The object of the membranes and membranous sacs was twofold; to reverberate, and thus intensify in the cavity the sounds, as is the case with the tympanum. Secondly, the small apparatus with which it was connected, and by which it could be tightened or slackened at will, was to alter the pitch, so that sounds might be heard which the uterus might convey, on a membrane as tense as would produce the note conveyed by the womb-sounds. This, however, in practice, was found in no way to act as I expected. Sounds appeared to be equally well transmitted without as with the membrane, while a double membrane seemed rather to muffle the sound than otherwise.

I have even used a metal vaginoscope with fluid compressed within it by a membrane, to try if I could feel thereby any wave of pregnancy or fluctuation conveyed.

In the present stage of my inquiries, I have come to restrict myself to two varieties—the glass stethoscope, and the wooden vaginoscope.

There is nothing new in the first. It has been used for a long time; and glass has a single advantage. It is a beautiful conductor of sound, and rather, I think, raises the pitch. At any

rate, it enables you to hear more distinctly than with the ordinary wooden stethoscope. Add to this, it is very readily kept clean, and does not make any noise when rubbed against the clothes. This, as I before said, is a great advantage, and allows it to be used very modestly; and when manufactured in the shape I now show you—that is, long, and with a curve—is very easy of application. These were made by Mr. Maddox of London.

The second variety includes the single and the double vaginoscope. These were made by Mr. Coxeter. You will see that the vaginoscope, as I use it, is nothing more nor less than either the single or double wooden stethoscope, with elastic tube, *to the distal end of which is affixed a wooden speculum.* Occasionally I use a solid tube in lieu of an elastic one, made either of gutta percha or of wood. I am satisfied, however, that, were this manufactured of glass, it would be preferable.

These, then, are the instruments. One word now about their application in diagnosis.

My first business was to ascertain what were the normal pelvic sounds heard in a non-impregnated woman. There may be none. Occasionally, where there is much uterine congestion or vaginal inflammation, you may hear a regular pulse. This is, however, rare, and is probably the vaginal pulse. Commonly, you hear the gurgling in the intestines, passages of wind like bronchial respiration, and of fluid. These sounds, curiously enough, I have not heard in pregnancy, except occasionally the pulse before referred to. I imagine this to be due to the non-transmission of sounds through fluid, which the uterus contains in the early as well as in the more advanced periods of pregnancy.

The murmur of fibrous tumours presents some resemblance to that of pregnancy. Having, however, so fully developed this subject in my Lettsomian Lectures, I need not dwell here on this subject. Suffice it to say, that the true *fibroid tumour* is tubular, and usually louder, and often accompanied with a thrill, and a single or double cardiac sound, synchronous with the first or both sounds of the maternal heart. The *placental murmur* is intensely vesicular, without thrill, and usually without the cardiac sounds conveyed from the mother, except under very exceptional circumstances.—*British Medical Journal, Nov.* 26, 1864, *p.* 593.

90.—ON THE SICKNESS OF PREGNANCY.

By Dr. James Patterson Cassells, Glasgow.

In the morning, an hour before rising, let the patient drink about three ounces of very strong, hot decoction of coffee, and

during the day the food must be liquid, taken in small quantities, and often. If this fails, try in conjunction five drops of tincture of iodine in a teaspoonful of cold water every two hours—highly lauded by Dr. Churchill, I think, at least if my memory serves me; so Dr. J. G. Wilson, of this city, told me when recommending its use in a case which resisted every other mode of treatment, but which was cured at once by a few doses.

Should everything fail to give relief, and all food taken be instantly rejected, what can be done? Nutrient enemata, very concentrated and frequently used, afford the only chance of quieting the irritable stomach. Such a plan may be used for many weeks, and in my hands has never failed to soothe the irritable stomach and continual nausea. After a period of from three or four days to as many weeks, food may again be very cautiously partaken of, and will in the majority of cases be retained without any troublesome symptoms. I have followed the latter plan of treatment in three or four extreme cases, and had no cause to regret doing so, the patients and children not having suffered any injury; while I am positive one patient at least would have died had she not been kept up by the enemata alone for sixteen days, nothing but a small quantity of ice having been swallowed during that time.—*Lancet*, *Dec.* 17, 1864, *p.* 707.

91.—ON SUPPORTING THE PERINEUM AS A PREVENTIVE OF LACERATION AT CHILDBIRTH.

By Geo. K. H. Paterson, Esq., Balbeggie, Perthshire.

In my own practice I have invariably acted on the principle of giving support immediately before and at childbirth, during more than twenty years, and have never regretted doing so, but rather am fully convinced that had I not given such support, I should have had a good number of lacerated perineums to witness and attend to. How the perineum can undergo less risk of being torn by withholding than by giving support to it previous to and during the expulsion of the head of the infant, I must confess I am as yet unable to understand. It must be allowed that laceration of the perineum at childbirth is equally to be expected amongst the wives of the Scotch peasantry as anywhere else; and having attended many of these during past years, of stout make, and of ruddy colour, the infants being generally large, it has always appeared to me that due and proper support prevented laceration of the perineum, even when it seemed most likely to take place; especially where the patient had been left unaided, and when the perineum had been long stretched, owing to the unyielding and unadapted state of the vaginal appendages and head of the fœtus, while the

expulsive pains were violent and recurred frequently; and it is oftenest in such instances, I presume, that the obstetrical practitioner may look for a lesion of the perineum as likeliest to occur, unless prevented by means of support applied with the bare flat of his right hand when the fœtal head is resting *firmly* on the the perineum, and coming forth under the continued expulsive uterine pains. When the propriety of affording support at this juncture of the case is convincingly felt by the medical attendant, his hand should be so applied as to compress and include within it the vertex of the fœtal head, and also to pilot it if anything a little forwards; and while it is emerging quickly from the vulva, he should keep the hand steadily in apposition, and at the same time depress backwards gently the perineum over the face and especially the chin of the infant as it is being expelled into the world. There is often a difficulty and danger to the perineum in certain cases, and far more so in my mind when unaided, in getting it safely clear of the chin when towards the sacrum; and no better method have I found than the above so as to prevent laceration from occurring. In the married and unmarried country females and out-of-door workers, consisting of those of stout make, and also of little stature, who bear children very frequently, this mode of giving support is undeniably of great advantage; but I make no exception, as I apply it in all my cases of head presentation. And I do think in regard to supporting the perineum, that its advantages ought to be more generally appreciated, as in the rapid and violent expulsive pains, accompanied with large and hard or ossified head and rigid perineum, vagina, and vulva, I am confident by keeping with the hand well applied the head of the fœtus as often as possible back a little, instead of hastily delivering when pressing strongly on the perineum, that I have saved several lying-in women from inevitable laceration; and such a procedure it is reasonable to suppose allows the perineum, &c., to expand more gradually, and renders the risk referred to less to be apprehended in consequence, even when most likely to take place. But truly I have never found it to occur when such preventive practice has been followed.

Although it is said that no labours are so easy, or so soon recovered from as those of the wives of wandering mountaineers in barbarous countries, still their whole lives are so dissimilar to civilized life in this country, that little good seems derivable from a comparison of lying-in-women under such different circumstances; and I do not think that the non-occurrence of rupture of the perineum among the former should be an argument for our withholding support to the perineum among our own patients.—*Edinburgh Medical Journal*, *Nov.* 1864, *p.* 419.

92.—ON THE ADVANTAGES OF A MORE EXTENDED USE OF VERSION IN CASES OF DISPROPORTION.

By Dr. Andrew Inglis.

[The increased use of turning is one of the principal means by which the employment of craniotomy has been and may be still further diminished. It is best that in all head presentations, in which the forceps will not succeed, turning should be attempted, even though there is no hope of extracting by means of it alone.]

The advantages of such a use of version seem to be the following:—1st, That, by enabling us to ascertain definitely the proportion between the size of the child and that of the passages, it may prevent recourse being had to unnecessarily severe operative procedure, which might otherwise be resorted to from ignorance of that proportion. 2nd, That in cases which, without turning, would require craniotomy, the application of forceps to the head, *after turning*, will often be sufficient to effect delivery with a better result to the mother, and frequently without injury to the child. 3rd, That, even in the cases where version does not enable us to avoid craniotomy, its previous employment seems to render that operation easier of performance without adding in any degree to the risk.

There can be no doubt with regard to the first advantage, viz., its usefulness as a means of diagnosis, enabling us to avoid needless severity of operation, because it is manifestly the only method by which the *relative* size of the child to the passages can be determined; and, in the absence of such evidence, mistakes, proving serious both to mother and child, have often occurred even in good hands. I may mention, in illustration of this, that in cases of turning, where nothing else than craniotomy could have otherwise been performed, the child has often been unexpectedly extracted alive. Moreover, there have been cases in which craniotomy has been performed, although the deformity was so great as to detain the breech of the child, and the result has been, as far as I have been able to discover, uniformly fatal. Some cases also have been recorded where, after craniotomy had failed, the Cæsarean operation had eventually to be performed. Now, in these cases, had turning been attempted, as a means of diagnosis, it would have been ascertained at once whether the breech could pass or not, and thus a great amount of time would have been saved, and unnecessary injury avoided, and the propriety of Cæsarean section at once discovered.

The second benefit gained I have stated to be, that the use of forceps with turning as a preliminary, may often enable us with advantage to avoid craniotomy. Such a use of forceps has

never found its way into general practice, although there has been good authority for it. Smellie was the first to propose it, and when he did deliver by this method, his results were highly satisfactory. Dr. Granville of London has followed his example, and found the forceps so useful in such circumstances, that he has twice recorded a series of successful cases. I have also met with others who, from their success in this mode of operating, strongly advocate it.

The third advantage which I have stated to be derived from turning, is that, should the forceps fail to procure delivery, or appear inadmissible, the operation of craniotomy will be more easily performed *after turning*, and with probably less risk than if the head were presenting. I have been led to form this opinion principally from the good recoveries cases are generally reported to have made where craniotomy has been performed after extraction of the body, and also from the invariable shortness of the description given of the operation. Dr. Ramsbotham, in speaking of craniotomy under such circumstances, says, that perforation alone is generally sufficient; and in only a very few cases have I heard of the head requiring to be further broken up. It has often happened that, after turning, the extraction of the head entire has been found impossible; but the description of the operation required to complete delivery has, in every report I have been able to find, been very concise, leading to the inference that no great difficulty was experienced. As an instance, I may refer to the first volume of the Transactions of the London Obstetrical Society, where the description of a case runs as follows:—"The occiput was perforated, the brain washed out, the cranial bones crushed inwards, and the head extracted. The placenta followed, the bleeding ceased, and the patient recovered without an ill symptom." The late Dr. Ramsbotham met with a number of cases of the kind, and yet in recording them, he gives us no reason to suppose the operation to have been rendered either more difficult or more dangerous by the malposition of the head. Smellie also seems to have found it an easy operation, if we can judge from the contrast between his lengthy details of the ordinary form of craniotomy, and his concise and simple accounts of the use he made of his crotchet after the birth of the body.

On the other hand, when craniotomy is done on the head while presenting, great difficulty is often met with, and many hours have often been spent on the operation, even where the pelvis was so wide as easily to permit the subsequent passage of the body. In addition, I would point out the fact, that cases have been related where craniotomy alone having failed to effect delivery, turning has succeeded in procuring extraction with such case that we must conclude that, though the head had not

been sufficiently broken up to permit its propulsion through the pelvis, it had yet been broken up much more than was required to allow its being drawn through after the body.

From all this I infer that, instead of adding to the danger of the operation of craniotomy, turning must diminish it, by enabling us to dispense with an amount of breaking up of the head which, *per se*, is much more likely to do harm than the "operation" of turning. There are many who, from their experience of version under all circumstances, assert that there is at least very little danger to be apprehended from the operation when properly performed, even without chloroform to quiet the uterus. Dr. Ramsbotham states, that he never saw any bad results from even the most difficult cases of turning; and his employment of the operation has been considerable. I may also refer to Dr. Figg's well known views and experience. Moreover, Mr. B. Hicks has improved greatly on the old method of operating, which had already been considered highly satisfactory. I have turned frequently for malpresentation, and find that, where the pelvis was normal, the mother has recovered much better than after an ordinary presentation. In some, where the pelvis was so contracted as to prevent their having a live child without turning, the results have been quite as good. Also, in the few cases I have had where the deformity was such as to require traction so great, that one or two of the bones of the pelvis and trunk of the child gave way, the recovery has been invariably as rapid as after an ordinary labour. In such cases I have never hesitated to use a great deal of force in drawing the body through, with the full confidence that, even if it came to the worst, the head would be more easily and safely extracted in that position than if acted on when presenting in the usual manner. The reason so much less fracture is required after turning than before, seems to be, that, as the occiput is the greatest obstruction, and requires to be fractured as near the neck as possible, when craniotomy is performed on the head presenting, one or both parietal bones, the greater part of the occipital, and often the frontal, have to be cut away to allow of that point being reached; whereas, after turning, it is the nearest point, and perforation alone should almost always be sufficient to allow the bones to fall in as required. If, on the other hand, more than simple perforation be necessary, the hold afforded by the body should be a great assistance. Besides, it must be recognised as of the utmost importance that, in delivering the head, the force applied is *traction* at the centre of the resisting part, while in cases where the head presents, it is *propulsion* by a hook inserted behind the head, involving either the employment of a much greater amount of force or a much greater extent of fracture. An idea

of the comparative merits of the two operations may be obtained by the following illustration. If we take a hollow india-rubber ball and try to *press* it through a round aperture a little smaller than its diameter, we shall find that it will flatten out and thus *increase* in diameter; and, if we make an incision in the part protruding, it will flatten out still farther; but if, on the other hand, we attempt, to *pull* it through by means of a string attached to it, the ball will elongate, and so *diminish* in diameter; and, if incised, will collapse, even although the aperture be a small one, without farther limit than is imposed by the substance of the ball.

I have now given reasons for holding that preliminary turning lessens the danger of craniotomy by rendering the operation less difficult, prolonged, and extensive. I have further to state, that it lessens the danger by enabling us to perform the operation at a much more favourable time. In all cases where the forceps are unsuitable, or have failed to procure delivery, if turning is had recourse to, it will be performed at once, and thus craniotomy, if necessary, will be demonstrated to be so, and consequently resolved on and performed at a much earlier, and to that extent more favourable period of the labour than would be the case if the head were allowed to remain presenting.

Besides the danger resulting from exhaustion, &c., caused by the delay both previous to, and in the performance of craniotomy on the presenting head, there is, it must be noted, some risk of rupture of the uterus before delivery by the operation in question. If the uterus ruptures during labour, the lesion always takes place after the os is pretty well dilated; and a very large proportion of the cases of rupture have met with the accident either while the operator was waiting till the labour was sufficiently advanced for the performance of craniotomy, or during the operation. If, on the other hand, turning is applied to such cases, it will almost certainly be performed before the labour has advanced so far as to render a rupture of the uterus at all likely; and the body being once delivered there is no risk of such an accident during the extraction of the head, however long the operator may delay in breaking it up.

From these considerations, I would therefore strongly recommend turning to be attempted in all cases of disproportion too severe to permit of delivery by the forceps, unless the necessity of Cæsarean section be plainly indicated, with the full assurance that, if farther instrumental procedure be required, instead of complicating, the turning will be found to render extraction much more easy.

Moreover, I think, that those who admit that in cases of difficulty the head can be more easily extracted by version, on account of the advantage gained by the improved position,

combined with the increased power of adaptation consequent on that position, must concede that, if the power of adaptation be increased by subsequent crushing, the assistance derived from version must also in these cases be increased.

Finally, even though great improvements in the manner of breaking up the head should be introduced, still the use of version as a preliminary will continue to be a great assistance, and its advantages as a means of determining the cases in which the necessity for craniotomy exists must also continue to be of the utmost importance.—*Edinburgh Medical Journal, Dec.* 1864, *p.* 503.

93.—ON THE APPLICATION OF EXTREME COLD AS AN ANODYNE IN THE PAIN ATTENDANT ON PARTURITION.

By Dr. J. Mortimer Granville, Bristol.

[The author has formed the following conclusions regarding the nature, duration, and intensity of the pain accompanying labour.]

1st. The actual pain (as distinguished from the sensation of forcing) experienced by the parturient woman bears no definite proportion to the force and efficiency of what (for want of a better name) we term "the pains" of her labour. 2nd. The sensation of pain is not invariably synchronous with the occurrence of uterine contraction, the effects of the latter being perceptible to the accoucheur not unfrequently, during examination, before his patient experiences the former.

From these circumstances, taken in connection with others well known to careful observers, it may be inferred that the pain attendant on labour is more directly related to the effects of uterine contraction (perhaps mechanical force operating on nervous tissue) than to the specific muscular act itself.

The reference of the pain to some region more or less remote from the contracting uterus or the dilating external passages (in which the seat of pain might have been supposed to be located) appears to support the last inference, whilst it clearly points to the conclusion that the pain attendant on labour is neuralgic in its character.

Acting on this theory, the author has tried the effect of extreme cold as a topical anodyne, and the result has been so constant that, after repeated experiment, he feels confidence in bringing this new method of seeking to alleviate the pains of labour before the society, and suggesting its adoption by the profession. The method is applicable in all cases, independent of the condition of the patient, cold being already in use as a powerful means of exciting the uterus to contraction in cases of extreme exhaustion. It is perfectly free from the dangers so

inseparable from the most cautious use of anæsthetics. It is perfectly manageable, and may be safely confided to a nurse; and its *rationale* is in strict conformity with the well-known principles of physiology. It is not intended to remove, but to alleviate, the pain attendant on parturition. It will be found that the power of the uterine efforts is generally increased, without any corresponding increase in the attendant suffering, the patient being not unfrequently under the impression that the pain has ceased when the presentation continues to evidence the continuance of the expulsive force. This has been very remarkable in some of the cases, the pains appearing to be shortened when examination has proved them to be prolonged. The actual pain is sensibly diminished, the patients expressing themselves variously, but to the general effect that they are relieved of half their suffering.

The method of the application is of the greatest importance. The cold must be extreme, or it is of little, if any, service, and it must be persistent.

An apparatus was exhibited, consisting of a flat tin box, which is filled with a freezing mixture and applied to the seat of the pain.—*Obstetrical Transactions, Vol.* vi. 1865, *p.* 105.

94.—DESCRIPTION OF THE "GUIDE-HOOK," A NEW OBSTETRIC INSTRUMENT.

By Samuel Newham, Esq., Bury St. Edmund's.

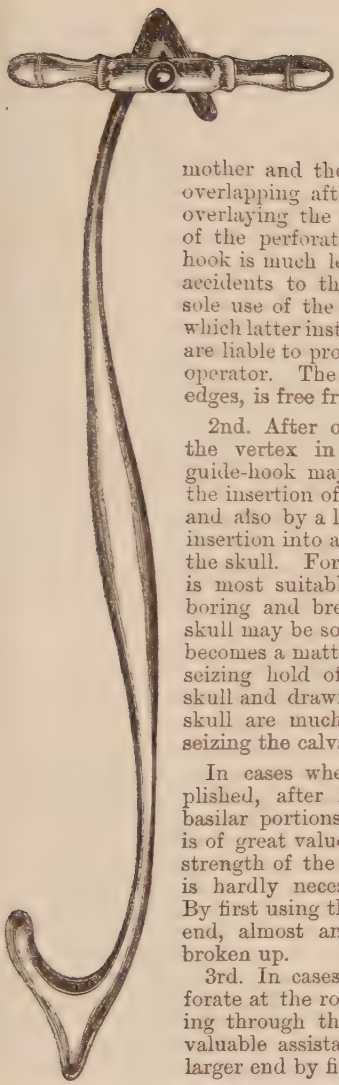
This invention is simply a blunt-hook, with the addition of a prow or guide.

The addition of the guide to the hook is for the purpose of favouring the adjustment of the hook into many positions in which a hook is required. It is not intended by this instrument, however, to supersede the use of the ordinary blunt-hook, for the blunt-hook may be used under circumstances in which it might not be safe to use the guide-hook. It is intended to supplement many purposes which the blunt-hook is not constructed to carry out.

The purposes for which the guide or prow will be found useful are:

1st. The insertion of the hook into any opening, whether a natural one, or one effected by the previous use of any other instrument.

After perforation of the head, not only is the hook with the addition of the guide more easily introduced, but the larger guide itself may be used as a means to dilate the opening and break up the bones, and thus freely to discharge the contents of the skull, obviating in this way a prolonged use of the perforator.



In comparately undilated conditions of the os—in very contracted pelvis—or in any position of the fœtus where it is necessary to introduce the hand far within the mother and there is probability of the bones overlapping after perforation, or of the skin overlaying the artificial opening, a slight use of the perforator and free use of the guide-hook is much less liable to the production of accidents to the uterus or vagina than the sole use of the perforator and crotchet, both which latter instruments, having cutting edges, are liable to prove hurtful to both patient and operator. The guide-hook, having no cutting edges, is free from this liability.

2nd. After opening the head at or about the vertex in ordinary presentations, the guide-hook may be made use of to extract by the insertion of it into the foramen magnum, and also by a little force and rotation, by its insertion into any of the bones of the base of the skull. For this purpose the smaller end is most suitable, and by a repetition of this boring and breaking process the base of the skull may be so disintegrated that extraction becomes a matter of ease. By thus piercing, seizing hold of the basilar portions of the skull and drawing down, the contents of the skull are much more easily ejected than by seizing the calvaria by the crotchet.

In cases where turning has been accomplished, after first perforating any of the basilar portions of the skull, the guide-hook is of great value in breaking up the arch and strength of the bones. In many instances it is hardly necessary to use the perforator. By first using the smaller and then the larger end, almost any part may be entered and broken up.

3rd. In cases where it is required to perforate at the roof of the mouth, or in thrusting through the orbits, the guide-hook is of valuable assistance—preceding the use of the larger end by first inserting the smaller.

4th. In cases of a symmetrical pelvis the guide-hook is of great service in turning the direction of the head into a more convenient axis—that is, in directing the occiput to the wider side—the guide, for this purpose, being thrust into the base of the skull, and then rotated.

5th. In perforating or thrusting into the thorax, whether before or after the removal of the arm, the guide-hook is also of value.

6th. In cases of evisceration through artificial openings, or by insertion of it into the anus, the guide-hook much facilitates every proceeding.

Of course there are circumstances in which it is necessary to use a hook in which the guide might be an encumbrance, or even an obstruction, but the care which an adept operator would always exercise in the use of any instrument would reduce these circumstances to very few in number. In many instances a hand or fingers may be used in advance of the guide, and no fear of accident need be dreaded.

I feel great satisfaction myself in possessing so useful an addition to my *armamentum obstetricum*, and confidently advise every accoucheur to add the guide-hook to his collection of instruments.

P.S.—I have had also constructed a handle or key to fix on the smaller end of the guide-hook, to enable the operator to act with force when the larger end is inserted in any part which is very solid or difficult to move.—*Obstetrical Transactions, Vol.* vi, 1865, *p.* 7.

95.—A RARE CASE IN MIDWIFERY PRACTICE.

By George Rigden, Esq., Canterbury.

On the 2nd of August of the present year, Mrs. G., aged about twenty-five years, of a leucophlegmatic temperament, and residing in this city, was taken in labour with her second child at about five o'clock in the evening. The pains were reported to have been frequent, but not very strong until about eight o'clock, when my attendance was requested. Upon examination, the forehead was found presenting, and partly entering the brim of the pelvis, with the os uteri moderately dilated and dilatable. The patient was extremely anxious in consequence of having experienced a very protracted labour at her first confinement; but there appeared no reason to doubt but that upon this occasion it would proceed more favourably. The pains continued frequent and strong, but with scarcely any effect upon the progress of the labour until half-past nine o'clock, when they almost suddenly ceased; and, not returning by half-past one in the morning, the forceps were applied, and the labour

quickly terminated by the birth of a full-aged living child. A broad bandage was immediately applied round the lower part of the abdomen, and the right hand passed between the thighs to the region of the uterus, which was distinctly felt, and moderately contracted. After about half an hour's interval, the uterus was again felt in the same position, and in about the same state; and, as the placenta could not be reached through the vagina, the patient was directed to make a slight expulsive effort, which immediately changed the whole aspect of affairs. The uterus passed away from the hand above the pubes, and was found to have descended through the vagina, and to be completely inverted between the thighs of the mother, with the placenta still attached to the greater part of the larger and more dependent part. No traction had been made at the cord, and the inversion seemed due entirely to the moderate expulsive effort of the mother. The placenta was immediately and quickly with the ends of the fingers peeled off from the uterus, and the latter at once, and without the slightest difficulty, replaced in its natural position. After retaining the hand in the vagina for a few minutes, the uterus was felt contracting, and was also distinctly perceptible above the pubes. The patient was directed to remain in the same position for several hours, and not on any account to make an expulsive effort.

It is satisfactory to record that the uterus remained in its natural position, and that the patient made an excellent recovery without any other bad symptom. She was desired to be particularly careful of herself for several weeks. It is now more than a month since the confinement, and she does not experience any bearing-down or other unpleasant effect from what cannot but be considered as having been a very dangerous complication.—*Lancet, Dec.* 17, 1864, *p.* 689.

96.—AN ENQUIRY INTO THE BEST MODE OF DELIVERING THE FŒTAL HEAD AFTER PERFORATION.

By Dr. J. Braxton Hicks.

In introducing the subject of this paper, the Author said that, notwithstanding the employment of premature labour and version, cases would occur in which it was either necessary or desirable to perforate. He pointed out that the subject had of late years not received the attention it deserved. He alluded to the disputes which arose upon Dr. Osborn's case of E. Sherwood, when that physician asserted he could draw a child's head through a brim having an inch and a-half antero-posterior diameter, by tilting the base of the skull sideways, and concluded that Cæsarean section might be done away with.

The disputes which followed were so acrimonious that the valuable points elicited by Drs. Hull, Hamilton, and Burns were, to a certain extent, lost sight of, at least as far as they were calculated to give any rule in practice. Dr. Burns in particular deduced from his experiments that the calvarium of the fœtal head being removed, the base of the skull could be drawn down easier face foremost than in any other direction. With this the author's experiments entirely agreed. And he pointed out further the advantage of the chin pointing anteriorly during the descent. He further instituted a comparison between the opposing diameters when the face is made to present, and the other modes of drawing down the base of the skull. He then proceeded to answer the inquiry: If in cases of extreme lessening of the antero-posterior diameter, it is best to cause the face to present; and if, after simple perforation, it is best to continue vertex presentation, at what degree of reduction of the size of the head do the two presentations cause equal obstruction? This he answered by the results of experiments, which might be thus concisely stated. That, as is acknowledged by all, vertex presentation in natural labour is the best; and that after perforation and evacuation of the brain up to the extent of one-fourth, this rule holds good; yet if the evacuation of the brain and collapse of the calvaria by this means, or by more or less fracturing the bones, be carried to a greater degree, we find that the facial presentation affords the easiest mode of delivery, provided that the mentobregmatic falls beneath the bizygomatic diameter. And, further, that if we remove the whole calvarium, leaving merely the base, and then induce face presentation, taking care that the chin, as it descends, points anteriorly, we diminish to the smallest possible amount, short of wholly breaking it up, the opposition of the head, leaving only from one to one and a-half inch in depth to oppose the conjugate diameter of the brim, and from three to four inches at the outside to oppose the transverse. The author, as practical deductions from these facts, recommended that in cases where simple perforation failed, to allow the descent of the head in cases of obstruction—say above three inches antero-posterior diameter—to break up purposely and carefully the bones of the calvarium, and remove at least a portion, preserving the scalp as protection to the edges, and then to induce face presentation. That when the diameter was under three inches, then to remove all the calvarium, and then to induce face presentation, taking care to bring the chin forwards, if not already in that direction. Dr. Hicks then pointed out the facility of doing this with a small blunt hook, which could be readily, and without chance of injury, passed up to the orbit. The chin he had found, had a tendency to point anteriorly upon being drawn down. He

then entered upon some useful details, and compared this mode of craniotomy with the cephalotribe. He remarked that by this means, in deciding upon whether craniotomy or Cæsarean section should be performed, the head was not so much to be considered as the size of the body in cases of brim obstruction. The paper was illustrated by eight cases of craniotomy, six of which were required for contraction of the conjugate, and two for obstructions in cavity. In all the induction of face presentation was attended by instant and complete passage through the obstacle. In some of the cases the shoulder and pelvis of fœtus gave more difficulty than the head.—*Medical Times and Gazette, Jan.* 28, 1865, *p.* 103.

97.—DESCRIPTION OF AN APPARATUS FOR THE PERFORMANCE OF TRANSFUSION.

By Dr. Graily Hewitt, Physician to the British Lying-in Hospital; Assistant-Physician-Accoucheur and Lecturer on Midwifery at St. Mary's Hospital.

The apparatus is constructed to carry out the operation of transfusion in the following manner:

The blood to be transfused is received into a glass syringe, and by means of this thrown into the vein of the patient requiring the blood. The method adopted allows of the almost instantaneous transference of the blood from one individual to another, and the exposure of the blood, which, as is well known, tends to produce rapid coagulation, is reduced to a minimum. This is effected by making the syringe itself the vessel into which the blood is received. For this purpose the piston of the syringe is removed, and the barrel of the syringe is held in an inverted position over the orifice of the vein, in close apposition with the surface of the skin, but not pressed too firmly, otherwise the blood will not flow. The blood then flows into the syringe, and when a sufficient quantity is collected therein the syringe is suddenly removed and the piston inserted, the blood being prevented from escaping by inserting the little plug into the escape-pipe. The syringe is constructed to hold two ounces.

Prior to filling the syringe with blood the arm of the receiving patient must be prepared. For this purpose the arm is to be uncovered, the vein selected which is largest—usually the median-basilic or the median-cephalic—and a longitudinal incision, one and a half inch long, made through the skin immediately over the course of the vein. The vein itself is then to be dissected out, and completely bared for nearly one inch of its course. The vein is then to be seized with the forceps and a slight incision made in it by means of the scalpel. This inci-

sion should be made obliquely, and just enough to admit the canula with its plug. The canula at the extremity of which the rounded plug projects, is slightly curved. The next step is to introduce this canula, with its plug, into the vein; the plug is then withdrawn, and the escape of a minute quantity of blood shows that the canula is properly inserted. The arm is then to be carefully and steadily held by an assistant, with directions to prevent the escape of the canula from the vein. The operator next procures the supply of blood in the manner previously described, being careful not to proceed to this stage of the operation until everything is perfectly ready for the injection of the blood. Experience has shown that the operation will most frequently break down from want of attention to this rule. The actual injection of the blood is performed as follows:—Taking the syringe with its contents to the bedside as rapidly as possible, the operator removes the little plug from the canula, and adjusts the syringe to the latter. The adjustment can, owing to the construction of the apparatus, be effected instantaneously, and by gently depressing the piston the blood flows into the receiving vein. The quantity thrown in at once should not exceed about one ounce or one ounce and a-half, and the injection of this quantity should occupy about one minute. It may be necessary to throw in two or more such quantities, according to circumstances. If more than one syringe-full is required, the second or following supplies must be procured and injected in a manner similar to the first.

The glass syringe, with its adjuncts, admits of being very rapidly and easily cleansed. The "Transfusion Case" made by Messrs. Savigny, of St. James's Street, contains, together with the springe, two canulas with plugs, a small pair of dissecting-forceps, a sharp-cutting scalpel, and a common lancet. The bulk of the whole is very inconsiderable, and the case, containing, as it does, everything possibly required for the performance of the operation, can be carried in the ordinary "obstetric bag," being thus always at hand in case of emergency.

For the performance of the operation it will be found necessary to have the assistance of two individuals—one to hold the arm of the recipient, the other to manage the arm of the person supplying the blood. The incision into the vein, in order to procure blood, should be a tolerably large one, to ensure a rapid flow of blood into the syringe. It is not necessary, as was formerly supposed, to heat the syringe, or to take steps for maintaining the blood at a high temperature; the syringe should, however, be placed in a basin of clean water slightly warmed, in order to render the motion of the piston easy.

The object kept in view in the construction of this apparatus

is the rapid transference of blood from one person to another. Experience has shown that, while the rapidity with which coagulation occurs varies in different persons and at different times, two or three minutes may be always calculated on before coagulation will occur. The various steps of the operation must be carefully considered before undertaking it, in order to allow of the transference being effected within this interval of time. The tendency to coagulate is materially diminished by the method here adopted of receiving the blood, whereby the exposure to air and contact with foreign bodies is reduced to a minimum.—*Obstetrical Transactions*, *Vol.* vi. 1865, *p.* 136.

98.—ON A MEANS OF ARRESTING FLOODING AFTER LABOUR AND ABORTION.

By Dr. Robert Barnes.

[The following remarks were part of a criticism of a paper on puerperal fever by Dr. Snow Beck. The question was as to the best means of checking the hemorrhage which so much predisposes to subsequent absorption of putilage.]

As a means of preventing the loss of blood he had found nothing of equal efficacy to the injection of a solution of perchloride of iron into the uterus after clearing out the cavity of placental remains and clots. He had used this plan for several years, and in a large number of cases after labour and abortion, and had always had reason to congratulate himself upon the result. The perchloride of iron had the further advantage of being antiseptic. He had certainly saved several women from death from flooding, and believed these and others had been rendered less liable to puerperal fever. The plan he adopted was to carry in his "obstetric bag" a saturated solution of the salt. Of this solution he used one part to eight of water. But he thought that even a weaker solution was sufficient. It instantly coagulated the blood in the mouths of the uterine vessels.—*Lancet, April* 1, 1865, *p.* 341.

99.—ON THE CONSTITUTION OF THE PREGNANT WOMAN AS A CAUSE OF PUERPERAL FEVER.

By Dr. Robert Barnes, President of the Obstetrical Society of London, &c.

The conditions of the puerperal woman predisposing to fever are constitutional and local. The constitutional conditions arise out of, or are the culminating expressions of, conditions beginning with pregnancy. The local conditions are chiefly the

immediate results of labour. I propose, first, to trace rapidly the constitutional conditions of the pregnant woman.

Pregnancy often induces a profound alteration in the quality of the blood, and this alteration is necessarily attended by modifications of nutrition and innervation. This primary fact has always been recognised. Hippocrates and Galen saw in the blood of pregnancy such impoverishment as to lead them to regard it as approaching to a pathological condition. Mauriceau called pregnancy a disease of nine months. Sauvages, in his Nosography, classifies pregnancy amongst the cachexies. Boerhaave said, "Fœmina plurimis afficitur malis ex solâ graviditate oriundis." Baillie looked on pregnancy as "a state analogous to inflammation;" the decidua he likened to false membrane, and the liquor amnii to effusion of serum. The Italians of the present day bleed pregnant women, simply because they are pregnant.

The changes wrought in the blood in consequence of the parasatic maintenance of the embryo are partly the result of organic or chemical operations, and partly of chemical pressure. It is probable that the first, or organic, operations act very early in pregnancy. At least, I have seen convulsions with albuminuria as early as the fifth month, before the uterus was large enough to exert any material pressure. The first blood-changes, then, take place under the influence of the nutrition and development of the embryo. The circulating fluid, the circulatory apparatus, and the excretory organs have double work to do. They must suffice for two organisms. The blood must nourish both; it must purify itself of the degraded materials resulting from the molecular changes going on in both. For two reasons the rapidity of the circulation is increased. First, the quality of the blood being lowered, it must be sent round more quickly in order to compensate for its lesser value. Secondly, there is the increased demand: the more active *vis à fronte* caused by the new organism. There is a local stimulus and attraction in the growing uterus, causing enormous enlargement of the vessels more immediately supplying this organ, analogous to that which John Hunter observed to attend the growth of the antlers of the stag. There is every reason to suppose that so powerful a local action, even physiological, cannot go on without affecting the chemical properties as well as the volume and circulation of the blood. The pulse is quickened. The heart has to labour harder to maintain this increased rate of circulation. What is the consequence of increased functional activity in a muscle? Hypertrophy. That there is a normal, a physiological hypertrophy of the heart in pregnancy analogous to the physiological hypertrophy of the gravid womb, has been demonstrated by Larcher (1859), Guillot,

and others. As pregnancy advances, a third cause of hypertrophy of the heart comes into play. This lies in the increased mechanical hindrances to circulation from the diminished capacity of the chest, and the local impediments in the abdomen and pelvis. Associated with this hypertrophy of the heart is the exophthalmos, and enlargement of the thyroid gland, first described by Parry (1825), by Graves (1843), since by Stokes, Basedow, Beau, Natalis Guillot, Laycock, and others, and of which I have myself seen several marked examples. The peculiar condition of the blood is further marked by the tendency to osteophytic deposits in the cranial bones, and on the decidual surface of the placenta.

Thus we have two new conditions in the pregnant woman: degradation of the blood, and increased size and activity in the heart. These conditions influence every part of the system. Let us review the alterations in the blood and some of their consequences. Andral and Gavarret found that from the first to the sixth month the fibrin was below the physiological mean, and during the last three months above the mean; at the same time there is a diminution of globules. Becquerel and Rodier say the density both of the defibrinated blood and of the serum is diminished; that the water, the fibrin, and the phosphorized fat are increased, while the globules and albumen are diminished. Probably if organic chemistry were more advanced there would also be found various excrementitious substances. It is the opinion of Mr. Simon that the fibrin is excrementitious. The general result is hydræmia—a condition related to chlorosis or scurvy. The watery state of the blood is revealed in various ways. Jacquemier says he found a bruit de souffle at the heart in one out of four cases of pregnancy. Dubois, however, found it in only one per cent. Caseaux attributes the abdominal souffle, usually considered placental, to alterations of the blood similar to those of chlorosis, and partly to pressure. Against this theory, however, I must set the fact that I have several times heard a distinct souffle on one side and an ordinary arterial sound on the other.

The primary and apparently physiological alterations in the blood readily pass into pathological excess, and may give rise to, or be associated with, severe diseases.

The liver may be overpowered. It may prove unequal to the eliminative strain thrown upon it. *Icterus gravidarum* may ensue.

The kidney may be unequal to its work; hyperæmia, albuminuria follow. Sugar is not uncommonly found.

I believe that the heart may become morbidly hypertrophied, and soften from fatty degeneration. Palpitations are common.

I have also observed that the lungs, having excessive decar-

bonising or other eliminative work, and being irritated by excrementitious matter brought to them, may become affected with bronchorrhœa, or a form of chronic bronchitis.

The veins in distant parts of the body become liable to dilatations, and thus we have local retardations of the blood, and sometimes thrombosis. Œdema is another result. This is certainly not always due to pressure, and may occur independently of albuminuria. It usually increases with the advance of pregnancy—that is, with the increase of spanæmia, and of pressure upon the lungs. Hence with the increased effort to derive the blood through the pulmonic system, there must be compensating retardation of the systemic circulation. This is relieved by œdema.

Some diseases of the circulation and respiration resulting from pregnancy have been elaborately described by Dr. de Cristoforis (Annali Universali di Medicini, 1863). Perhaps he attaches too much importance to the mechanical pressure exerted by the gravid uterus upon the principal arterial and venous trunks. His views, nevertheless, are to a great extent well founded, and are highly suggestive. Certainly the influence of mechanical pressure in aggravating the effects of altered quality of the blood cannot be overlooked. The following are some of the propositions stated by Dr. de Cristoforis :—

1. The gravid uterus presses upon the principal arterial and venous trunks. There results an obstacle at the point of pressure, in consequence of which the arterial blood accumulates in the upper parts, and can only pass with difficulty to the lower parts of the body. This results in mechanical *superior arterial hyperæmia*, whilst the venous blood stagnates in the veins of the lower limbs, producing *inferior œdema*.

2. Circumstances which increase the volume of the uterus, as twins, dropsy of the amnion, augment the pressure and the results.

3. The consequences of superior arterial hyperæmia are—passive plethora, visceral congestions, which in their turn induce special morbid essences (as eclampsia), or render chronic and acute diseases more severe (as pneumonia, bronchitis, phthisis), whilst the liver becomes voluminous and fat, and the spleen soft.

4. By the continuous effort to which the left ventricle is excited, there arises hypertrophy, and this in its turn, conjoined with the superior arterial hyperæmia, dispose to pulmonary and cerebral apoplexies.

5. The impeded respiration induces defective hæmatosis ; this impoverishment of the blood increases the disposition to œdema of the lung.

Thus we have all the disorders of the circulation, and all the

alterations in the quality of the blood, traced to mechanical pressure. That this is not altogether true is proved to me from having often witnessed serious impairment of the blood long before the uterus was large enough to exert any material pressure. But, in the latter months, I have certainly seen all the effects of pressure described by Dr. de Cristoforis; and there can be no doubt that the double and mutually aggravating disorder of the blood and disturbance of the circulating organs will, in many cases, induce serious constitutional affections, and predispose to puerperal fever.

Growing out of, and adding by a vicious circle of reaction to, these evils, is a disposition to physical languor and inertia. The secretions and excretions are all depraved or impeded; and the system suffers all the ills that spring from want of exercise.

Of course the summary here given of the disorders of pregnancy would form an exaggerated picture of the condition of the majority of pregnant women; but we may safely draw from it the conclusion that, in most cases, the cause of these disorders —pregnancy—existing, the effects will follow in greater or less degree.

But before labour comes, the constitutional conditions arising out of pregnancy have already developed a high susceptibility to the invasion of febrile and infectious disorders. I think I may state this proposition with confidence—namely, that the pregnant woman is far more liable to be attacked by small-pox, scarlatina, and typhoid, than the non-pregnant. It is also no uncommon thing for women received into lying-in hospitals some days, or even weeks, before labour to catch the prevalent puerperal infection. And the fact may be explained in the following manner. I am well satisfied, from repeated personal experience and observation of others, that persons in ordinary health coming within the range of zymotic poisons do frequently imbibe them. They are saved by two protective agencies. First, in their systems these poisons have previously undergone their specific fermentation. In the next place, their excreting organs, being in good working order, have the faculty of casting out the poisonous germs before they have time to do more than accomplish an imperfect stage of fermentation. But still they make their presence felt. Just like mineral poisons, they are carried to the liver, to the kidneys, to the intestines, to the skin, to the lungs. There they produce more or less irritation, and are eliminated. I am conscious of having taken on several occasions doses of choleraic, scarlatinal, varioloid, and other poisons, which have produced shivering, and then been carried out of the system by diarrhœa. The germ perishes and will not propagate. In like manner many offensive odours act. The odours imbibed by the breath are reproduced in the stools, in

which they may be perceived for days afterwards. There seems, then, reason to believe that the security against renewed fermentation of a zymotic poison in the same body is not obtained simply by a previous fermentation. I am disposed to think that the capacity of the system to throw off morbid poisons is increased by being frequently called into exercise. In this manner we may account for the comparative immunity enjoyed by medical practitioners under repeated exposure. There must, in addition, be integrity or sufficient power in the eliminative organs. Now it is precisely this eliminative power which fails in the pregnant and puerperal woman. Her excreting organs are overworked by the task of getting rid of her own and the embryo's excrementitious matters; and her blood is in consequence so degraded by its own impurities that it is incapable of dealing with the new poison received from without. Hence the foreign poison again finds a favourable medium for fermentation; and variola, scarlatina, or typhoid again run their course —even in a person who has had these diseases before. I think I am in a position to affirm that such is really the case; and that in pregnancy the susceptibility to take zymotic disease is so exalted that second and third attacks are more common, as they certainly are more fatal, than in other persons.

There are means of lessening this danger and of facilitating the discharge of these poisons. These consist mainly in generous living, so as to support the blood in the utmost efficiency; in exercise in the open air, which compels us to inhale a larger proportion of oxygen, which, coursing through the blood and adding to the energy of our excretory functions, especially of the lungs and skin, converts and throws off the noxious elements; in strict regard to cleanliness, especially bathing, which secures the activity of the skin. I believe it to be a grievous error for a medical man to indulge in a close carriage. To use a form of expression which is hardly a metaphor, by riding in the free air he may actually blow away the germs of disease which he has inhaled. If this be true as applied to persons in health, similar means are far more necessary to the pregnant woman. Active exercise she may be incapable of taking; but avoidance of close rooms and exposure to free air are almost always possible. The rules of hygiene necessary for all to observe who would have health are infinitely more important for the pregnant woman. She should live well; bathe once a week, to give the skin the power of compensating for the deficient activity of the internal excreting organs; take moderate exercise; secure good ventilation within doors, and be as much as possible in the open air.

No more striking idea can be formed of the abruptness and extent of the change undergone by a woman in passing from

pregnancy to child-bed than by stating the following fact determined by Gassner:—

By the act of delivery the pregnant woman loses one-fifth part of her weight. This is probably a larger proportion than is lost by the patient who undergoes amputation at the thigh. It is true that things have been preparing so that this sudden fission, as we may call it, of the parent-body may be effected with the least possible disturbance. The embryo has attained a stage of growth rendering it capable of independent life. But still up to the moment of separation, nearly all, if not all, its nutritive and excretory functions are performed through the mother's blood. Part of this loss of weight consists of the child, placenta, and liquor amnii. The combined weight of these may average twelve pounds. All the remaining loss must come directly from the mother's system. First there is the hemorrhage, more or less copious, which follows the expulsion of the placenta. In ordinary cases, where the uterus contracts fairly, the blood that so follows is, I believe, nearly restricted to that which was contained in the vessels of the uterus, and which is now squeezed out by the contraction of the organ. It is doubtful whether much of this uterine blood is driven back into the maternal vessels. It is superabundant, and is therefore cast out. But another, and a large part of the gross loss is thrown out gradually; that is, within a few days. It is this which puts the integrity and powers of the system to the test. The rapidity is marvellous with which the uterus passes from being the largest muscle in the body, weighing twenty ounces or more, to a weight of nine or ten ounces. The heart, too, loses weight, and an excess of blood and other fluids, and probably of solids, has to be got rid of. Now the organized solids, such as the muscular tissue of the uterus, can only be discharged by being in the first instance converted into soluble matter, which can be taken up into the circulation, and then carried to the excreting organs. Here, then, is an amount of work thrown upon the system unknown before. All that superabundant solid structure called into existence to build up the pregnant uterus has to undergo fatty metamorphosis, and the proceeds are rapidly absorbed into the blood. Thus the blood undergoes a new and extensive deterioration, to remedy which the utmost energies of the excreting organs are exerted. Hence we have commonly a greatly increased activity of the skin, manifested by perspirations and a peculiar odour, which shows that the skin discharges not water only, but effete organic matter; the kidneys also act freely, and not uncommonly an abundance of urate of ammonia and urea shows that these organs are discharging more than the usual excreta. The quantity of carbon and of watery vapour exhaled by the lungs

is also greatly increased; and here again the peculiar puerperal odour, the "gravis odor puerperii," reveals the fact that the lungs too are called upon to assist in throwing out the useless organic matter.

In addition to this active work of elimination, there is a new work of formation. The secretion of milk is taking place. It is probable that in the performance of these two functions there is some amount of reciprocal aid. Some of the materials resulting from the fatty conversion of the uterus may serve in the formation of milk. Certainly involution goes on best under the influence of lactation. If the woman do not suckle, the activity of absorption is impaired; the uterus remains bulky; and I am persuaded that it is to the neglect of this natural duty that many mothers may trace the origin of those uterine engorgements and hypertrophies which are developed many months afterwards. Indeed, Gassner appears to have reduced this proposition to actual proof. He found the loss of body weight was less in non-suckling than in suckling women.

Dr. M'Clintock, who has made valuable observations on the pulse in childbed, says that in a large proportion of cases of normal convalescence from parturition the pulse scarcely deviates at all from the standard of health. Where this is the case, it may be inferred that the excreting organs are doing their work fairly. Dr. F. Winckel found the temperature of lying-in women slightly higher than during gestation; and he, as well as Dr. V. Grünewalt and Dr. Traube, observed a marked and rapid rise in the temperature of the body whenever any inflammatory or febrile condition supervened. It is very rarely that things are so happily balanced in childbed that the establishment of lactation is effected without some degree of febrile movement. On the second or third day there is commonly increased frequency of the pulse and some heat of skin, sometimes even a slight rigor, attending the rapid filling and tension—a state often verging on inflammation—of the breast. This may be held to indicate a disturbance of the balance between the absorption of now superfluous matters and of their excretion. It is not, therefore, a "milk-fever," as it is commonly called. Nor is it, as some German authors describe it, a "wound-fever," the result of the uterine lesion. It is more, strictly, I believe, a fever resulting from the accumulation of effete noxious matters in the blood, which cannot be kept down by the combined agency of excretion and lactation. This is, perhaps, *the simplest form of puerperal fever*. It might be called "*Excretory Puerperal Fever.*" It is strictly autogenetic. Under favourable circumstances, lactation is fairly set up; the child stimulates and maintains the diversion to the breasts; the excreting organs, relieved by the assistance of the mammary

glands, recover their efficiency; the blood is kept free from the in-pouring adulterating matters; and the fever subsides in three or four days. But let one of three things happen—that is, throw into the system any extraneous poison, as that of a zymotic fever, or the miasm arising from the herding of three or four lying-in women together, thus increasing the burden weighing upon the excretory organs; impair the efficiency of the excreting organs by bad ventilation, cold, excessive heat, poor diet, want of cleanliness; or take away from the excreting organs the assistance of the breasts,—and the seeds of fever will burst into active germination, delirium set in, involution be arrested, and you may have to deal with puerperal fever in one of its most formidable shapes.

And there is yet another source of danger. The local violence sustained by the uterus and surrounding tissues by the act of labour constitutes a serious element in the puerperal state. It is this local injury which many eminent physicians abroad regard as the most potent amongst the causes of puerperal fever. The condition of the blood as resulting from the disturbed balance of absorptive, excretory, and milk-forming functions upon which I have dwelt, has attracted but little attention. Let us examine the immediate effects of labour upon the parts concerned in parturition.

The child expelled and the placenta cast, what is the condition of the internal surface of the uterus? Some—as Cruveilhier, Ferguson, Simpson, and Heschl—liken this inner surface, especially that portion to which the placenta adhered, to a surgical wound, such as that made by amputation. The muscular layer of the uterine wall, they say, is bared; large vessels are torn asunder; there is a breach to be repaired, vessels to be closed. The area first denuded by the fall of the placenta is almost instantly greatly reduced by the vigorous contraction of the uterus. This reduction in superficies may be generally stated as being from sixty to nine square inches. Still there remains a considerable lesion to repair. On the other hand, more minute observation tends to lessen the points of apparent analogy between the uterine wound and the wound made by the surgeon. Robin, Kilian, Dr. Matthews Duncan, and others, show that the muscular coat is not entirely exposed; that a preparatory formation of new mucous membrane takes place during gestation between the old decidua or mucous membrane which has merged in the placenta and the muscular coat, so that when the placenta is shed there is a new fine mucous covering ready to protect the muscular coat. But notwithstanding this view,—in the general correctness of which I concur,—it must be admitted that the detachment of the placenta and membranes does not leave a perfectly smooth and

clean surface. The old decidua adheres in shreds; clots of fibrin and blood project into the cavity, and here and there, as I have often ascertained by microscopical examination of aborted ova, of mature placenta, and of the uterus itself, the process of avulsion is so violent that not only decidua, but even muscular fibres, are torn away. We must, then, admit that there is a foundation for the comparison of a woman recently delivered to a patient who has undergone a surgical operation. Still I believe a too exclusive importance has been attached to the condition of the inner surface of the uterus. Other structures have undergone changes of considerable moment. I have never examined the body of a woman who has died within a few days after labour without seeing striking marks of the violence which, even under ordinary circumstances, attends that process. These effects are mainly traced in the cervix uteri, the os uteri, in the cellular tissue connecting the cervix uteri with the bladder, and in the cellular tissue of the broad ligaments. The expansion of the cervix is, in most cases, effected partly by the forcible protrusion of the child's head and body. The parts are violently stretched. The tissues undergo severe bruising, and even laceration. Small vessels give way. Ecchymoses, sometimes large effusions of blood,—the uterine hematocele of Dr. M'Clintock,—take place in the mucuslo-fibrous tissue of the cervix. For days after labour the flaccid lips of the os uteri, and the mucous membrane of the cervix, exhibit a deep slate-blue colour, almost black. In the peri-uterine cellular tissue I have always observed the following conditions:—The tissues loose; the interspaces large; and the whole infiltrated with serum, and sometimes containing ecchymoses, and even considerable collections of blood. Under certain conditions this state may pass into pelvic cellulitis, In addition to this there commonly remains great hyperæmia, and sometimes phlebectasis and venous thrombosis. The effused serum generally is quickly absorbed; but in the process it adds to the degradation of the blood. Here, then, we have a further local injury predisposing to local inflammation and general febrile reaction.

The enumeration of the sources of blood-contamination is not yet complete. Two factors remain to be considered. First, there is the shock of labour itself. This is both physical and emotional. It must be supposed that in the majority of instances the nervous impressions attending and following labour are beneficial. But in some cases it is not so. The shock to the nervous centres may be so great as to cause universal paralysis and sudden death. The effect of lesser degrees of shock in perverting the blood is undoubted. Sudden emotion is often the starting-point of puerperal fever. In other cases heart or arterial thrombosis is traceable to it. But a more con-

stant result of labour is the tissue-waste attendant upon severe and protracted physical exertion. This cause of blood-adulteration has not been sufficiently appreciated. Great mental tension and long-continued exercise of nerve-force entail rapid expenditure of the chemical elements of nerve-tissue. Dr. Bence Jones has shown that abundance of phosphates are formed in the urine after great mental exertion. In like manner great muscular exertion is attended by enormous expenditure of the chemical elements of muscle. Mr. Paget and others affirm that muscular action is always followed by the discharge of urea, carbonic acid, and water. Helmholtz shows that the muscles themselves, after long-repeated contractions, are changed in chemical composition. Dubois-Reymond has shown that the muscular juice, which, so long as the muscles are in a state of quiescence, possesses a neutral or faintly alkaline reaction, becomes, after they have been violently exercised, decidedly acid. Lactic acid is thus formed, and gets into the blood. Professor Ludwig again shows that arterial blood carried through a muscle in a state of contraction is almost completely deprived of oxygen. Dr. Edward Smith also has demonstrated that during great exertion the quantity of carbonic acid exhaled by the lungs becomes five times as great as it is during a state of repose during the day, and ten times as much as in repose at night. Brown-Séquard shows that the irritability of the muscles is extinguished by exertion. Thus in over-driven cattle, animals hunted to death, soldiers killed in battle, rigidity is early and short-lived, and putrefaction soon begins. It seems to me probable that the violent action of the uterus is the normal preparation for its involution. In this supreme effort the muscular tissue is wasted, softened, and prepared for liquefaction and absorption.

There are few occasions in life in which so much nervous and muscular energy is expended by a woman as in her first labour. She must then suffer in a high degree from the rapid conversion of nerve and muscle into waste matter, and endure from this source a further strain upon the excretory organs. I conceive this to be what takes place under the violent physical exertion of labour. A large proportion of water is thrown off from the blood; hence there is inspissation. There is accumulation of of carbonic acid in the blood; hence there is hurried circulation in order to make up for the imperfect action of the lungs. Aëration cannot keep pace with the progress of deterioration. Under these circumstances the conservative powers are easily overcome, and the heart circulates a fluid deprived of vital stimulating properties, and charged with effete irritating matter. The result is a condition analogous to that state of exhaustion which issues in typhus. The system trembles on the verge of

fever. I have known fatal pneumonia, pleurisy, and pericarditis ensue upon excessive physical exertion.

How important, then, is it to *shorten labour!*—that is, to cut short the waste of nerve and muscle, and to prevent unnecessary adulteration of the blood. Thus physiology emphatically condemns the old rule to let the head lie four or six hours in the pelvis before we interfere to release the patient from the torture of suspense and the agony of fruitless exertion. There can be no shadow of doubt that the timely use of the forceps may often save a woman from puerperal fever. I have also some reason to believe that in some cases chloroform, by removing the dread of pain and eliminating hurtful emotion, materially lessens the evils of excessive nervous action, and may thus indirectly lessen the danger of puerperal fever. It must not, however, be supposed that chloroform confers immunity against this disease.

To sum up the connexion between puerperal fever and the local and constitutional conditions derived from pregnancy and labour:—

1. Pregnancy induces a degraded condition of blood; throws an excessive burden upon the excreting apparatus; impedes the freedom of the circulation; causes hypertrophy of the heart.

2. Labour adds to the condition left by pregnancy: shock; extensive local injury; an enormous waste of nerve and muscle, the consequence of physical exertion; greater degradation of blood from this conversion of nerve and muscle, and also from the proceeds of the involution of the uterine tissues.

3. Hence a greatly exalted stress upon the excretory organs, and a general deterioration of the solids and fluids, inducing feeble physiological action, and therefore tendency to fall into pathological action; that is, to generate what in the strictest sense may be called Puerperal Fever.—*Lancet, March* 18 *and* 25, 1865, *pp.* 279, 307.

100.—ON THE PROPAGATION OF PUERPERAL FEVER BY ACCOUCHEURS.

M. Grisar has recently brought before the Brussels Academy of Medicine the subject of the communication of puerperal fever through the medium of the accoucheur. Although there is nothing new in the fact, it is of importance that it should be reiterated from time to time, in order to caution those who, unwarned, may prove curses instead of blessings to patients who seek their services. In place of recalling the testimony of the most celebrated accoucheurs on this point, we may cite M. Grisar's personal experience. In December, 1842, he delivered a woman with the forceps who died of puerperal fever on the

second day; and between then and the 19th of the following March, 16 out of 64 women delivered by him were attacked by puerperal fever, 11 of the number dying. As he found the disease did not prevail in the practices of other physicians, he came to the conclusion that he had been the means of communicating the contagious principle of the disease, and therefore took every possible precaution. Until the end of 1862, therefore, for twenty years, he did not meet with another case; but at the end of that year he had a fatal one; and between December 5 and January 26 following, of 9 women delivered by him 8 became the subjects of the disease, and of these 4 died. He had taken every precaution as respects ablutions, clothing, &c., but it was not until after he had suspended practice for a month that the disease ceased to appear among his patients. M. Guerin, commenting upon the above facts, observes that Chomel always called in Baudelocque to his wealthy patients, who, unattached to any maternity, he had found to be the only accoucheur of his day in whose practice puerperal fever did not appear; and more than one practitioner, renowned for his knowledge and talent, has acquired among the public a terrible reputation for the calamities which attended his presence. M. Guerin adds that he himself, as well as various other practitioners, have, during the prevalence of epidemics of puerperal fever, experienced symptoms which could only be explained by the presence of a poisonous miasm, and which may doubtless be transported, notwithstanding ablution, change of clothing, &c. Such persons suffer from general uneasiness, fœtid breath, eructations of a peculiar odour, and somewhat loose and strong smelling stools. They should observe the greatest cleanliness, freely breathe the fresh air, and repeatedly purge themselves. When the fever occurs in their practice they should make it a solemn duty to abstain for a while from attending other cases. —*Medical Times and Gazette, Dec.* 10, 1864, *p.* 625.

101.—ON EXCORIATED NIPPLES.

[There have been many replies in answer to a query in the 'Lancet' as to the best treatment for sore nipples. Dr. Mac Cormac, of Belfast, writes:]

My remedy, then, simply is the application—every time after the child has left the nipple—with a fine camel-hair pencil, of gum arabic in an impalpable powder. This powder, so applied, is perfectly bland and free from all irritative effects. It shields the nipple from atmospheric influences from the moment it is applied, and seems to secure the nipple from the excoriating effects attending the act of sucking. I shall be glad if other

persons derive from it the good effects which I experienced in the case adverted to.

[Another gentleman uses the following ointment:]

Take of semivitreous oxide of lead (litharge), half a drachm; acetate of lead, ten grains; spermaceti ointment, half an ounce. Mix.

[Dr. Eastlake writes:]

The moment an abrasion or fissure is perceived around the nipple, I recommend the *immediate* removal of the child from both breasts, and the application of the following simple ointment to the sore:—Honey, white wax, of each one ounce; almond oil, two ounces. Should the abrasion heal with sufficient rapidity to enable suckling to be continued *tant mieux;* if not, I believe it is far better to desist from nursing altogether, for by so doing we not only allow the sore nipple to heal, but we prevent the almost inevitable result of mammary inflammation.

[Mr. Storer, of Salisbury, recommends the following formula:]

Pure India-rubber, cut in thin pieces, one drachm and a half; muriate of morphia, six grains; pure chloroform, six drachms. Mix by shaking the whole in a phial, and let the nipples be painted over with the application night and morning.—*Lancet, Nov.* 26, *and Dec.* 3, 1864, *pp.* 623, 652.

102.—ON FIBROUS TUMOURS OF THE UTERUS TREATED BY SURGICAL MEANS.

By I. Baker Brown, Esq., Senior Surgeon to the London Surgical Home.

[Two papers on this subject have previously been published by Mr. Baker Brown (see *Retrospect*, vol. xliii, p. 318) and in which he gives an account of seven cases treated by the plan recommended by him. Of these seven, five were cured, one relieved, and one died. In the second paper he said "that the treatment only applied to intra-uterine fibrous tumours of the non-pedunculated form, growing from the inside of the uterus by a broad base." In his concluding remarks in the same paper he said,]

"1st. That intra-uterine tumours of the non-pedunculated form may be destroyed by a surgical operation, viz., by cutting into and destroying a portion of their tissue; in other words, by gouging a portion out." "2nd. That this operation may be performed successfully and safely. Out of the dozen or more

cases in which I have practised it, I have lost but the one which I have narrated in the present paper—a circumstance explicable as an accident." "3rd. That the preliminary division of the os and cervix uteri, in order to reach the internal tumour, has in itself the very important advantage of nearly or quite arresting the uterine hemorrhage, which is the most serious symptom attendant on uterine tumours."

Since the reading of this paper, some hundreds of professional men have witnessed my public practice at "The London Surgical Home;" and one gentleman, Dr. Routh, has lately stated in his 'Lettsomian Lectures' that he has successfully adopted the practice. My friend Dr. Dawson, of Newcastle-on-Tyne, has done the same.

My object in bringing this subject before the Society for the third time is to confirm by fourteen cases my previous good results, and, at the same time, to show that I have modified my surgical treatment; for whereas I uniformly divided my operative treatment into two parts, viz.—1st, preliminary incision of os and cervix; and 2nd, the gouging or breaking up of the tumour; I now find, by increased experience, that the first step is frequently all sufficient; and that even when gouging is required, a less severe operation than that previously adopted is effectual; and that, consequently, I never now use "Harper's instruments," but simply a pair of blunt-pointed scissors, finding that the slightest destruction of the capsule of the tumour is enough to ensure its disintegration.

I have been much gratified to find that Dr. M'Clintock, in his admirable memoir 'On Diseases of Women,' has confirmed the truth of my proposition, "that incision of the os and cervix does arrest the hemorrhage so frequently attending these fibrous tumours," and he gives two illustrative cases in his own practice.

Mode of Operation.—Let me draw the particular attention of the Fellows of this Society to the mode of performing the different steps of the operation, as on it I lay great stress both as to efficiency and safety.

Firstly, as to incising the os and cervix. The patient having been placed in the lithotomy position, the perinæum is well depressed by an assistant, by means of Bozeman's speculum. The anterior lip of the os uteri is then seized by a pair of vulsellum forceps, and held by the left hand of the operator, who then introduces the hysterotome, and, whilst firmly holding the forceps to the left side, cuts the os and cervix laterally to the right. Then reversing the hands, he holds the forceps towards the right side, and cuts out towards the left. The cut surfaces

are then plugged with oiled lint, and the vagina also thoroughly filled with lint or cotton wool, so as to ensure most perfect pressure and exclusion of atmosphere.

When this last stage is well carried out, there is never any serious hemorrhage; but, on the contrary, an unequal or inefficient plugging is often followed by hemorrhage, which may become in itself dangerous, or may be followed by inflammation of the uterus, and even peritoneum.

Hence it will be seen that the risk of this part of the operation is materially lessened by careful attention to the above directions.

Secondly.—Where it is considered advisable to perform the second step of the operation, viz., gouging the tumour (which should not be done *sooner* than a fortnight after incision of the os and cervix), the patient is again placed in the lithotomy position; the index-finger of the left hand of the operator is inserted within the inner os, and a pair of long-handled, blunt-pointed, curved scissors are passed up to the tumour, and gently pressed through the capsule; the blades are then so opened as to break up the tumour. It is not necessary or advisable to break up much. Oiled lint is then passed into the opening thus made by the scissors, by means of a uterine sound, and the whole vagina thoroughly plugged, as previously described. These dressings should not be removed under forty-eight hours, and, when removed, the vagina should be well syringed out twice a day with a lotion made of Condy's fluid, in the proportion of two drachms of the fluid to half a pint of water.

An opiate is administered to the patient by the rectum, a bland milk diet given, the most perfect rest and quiet enforced, and the urine drawn off every six hours by the nurse.

As I have already said, increased experience has proved to me that this second operation is frequently not required, for that incision of the os and cervix not only stops the hemorrhage, but also arrests the development of the tumour; in some cases the tumour decreases, and when small it will often entirely disappear. This last fact is more particularly proved where the tumours are of recent origin.

This is of great practical importance, and should induce us to wait for the full benefits of the first before we proceed to the second operation; and, indeed, unless the tumour be sufficiently large to interfere with the functions of the bladder, bowel, or uterus, and presuming that the hemorrhage has ceased, I would never advise any further surgical interference, but I do recommend that a steady medical treatment should be pursued, especially the use of the bichloride of mercury and bromide of

ammonium, having often seen the tumour go on decreasing, and even undergo the process of calcification, so well described by Rokitansky.

[Dr. ROUTH spoke in highly complimentary terms of Mr. Baker Brown's paper, and observed:]

He believed that fibroid tumours of the uterus, whether they were out of the pelvis or within it, required different treatment. But *neither* should be meddled with except vital functions were interfered with—above the pelvis, if the heart or lungs were pressed against; below the pelvis, if the defecation and micturition were interfered with; or in both cases, if the losses of blood were excessive and otherwise irremediable. But the operation of Mr. B. could not often be safely practised in supra-pelvic tumours, because, if the finger could not give proper indications, you might mistake the side on which the wall was fibroid, and cut through into the peritoneum, as had occurred in one case he knew of. The operation for such tumours, and especially if fibro-cystic, was gastrotomy, and in these cases it was best to remove veins as well as arteries, because risk of hemorrhage from a ligature on the cervix or vagina was so much less.

In regard to pelvic fibroids of the uterus, Mr. Brown had shown incontestably that a tumour, if cut through, would gradually absorb away. In two cases before recorded the tumours were partly projecting out of the os, and could be seen and felt quite distinctly. Now, ocular inspection in these showed that, after complete bisection, the tumours being left *in situ*, they disappeared altogether. The practice was therefore established to be proper in other cases, although the tumours were not visible. The opening of the os, as a preliminary step, both because it checked bleeding and enabled the operator to get more readily at the tumour, was wise, and, under the restrictions laid down by Mr. Brown, quite safe. He (Dr. Routh) invariably practised it.

[Mr. BROWN subsequently remarked:]

As to the *rationale* of the operation, he had avoided the question for fear of giving too wide a field for discussion. He thought the action was twofold. The tumours were of very low vitality, and the slightest interference with them stopped their growth. Thus, in the first place, the initiatory blood-vessels were cut through by incising the os and cervix; and, secondly, the tumour was grasped more firmly by the contraction of the uterus, which always takes place after the os is incised; and thus the tumour was confined, could not grow, and often died.—*Obstetrical Transactions, Vol.* vi. 1865, *p.* 22.

103.—CAULIFLOWER EXCRESCENCE OF THE UTERUS.

[At a meeting of the Obstetrical Society of London, a discussion took place as to the relative merit of different instruments.]

Dr. Routh said that, like his compeers, he, too, had used the wire-rope écraseur. Once, in using that of Dr. Hicks, he had found himself unable to remove the tumour by it, and had been obliged to have recourse to the chain écraseur to finish the operation. The same thing had happened to him in using Weiss and Co.'s improvement on Dr. Hicks's écraseur. The instrument had bent, and he could not cut off the tumour, and was again obliged to finish it by the chain écraseur. This, in private practice, was a great drawback. Some tumours were unusually hard, and required an unusually powerful instrument. It was right, however, to add that he had since, by an experiment, tested the relative powers of the double-action chain écraseur of the French and Weiss's wire-rope instruments, upon a cow's womb, previously hardened in spirit for days. Here the instrument of Weiss had the decided advantage. The part was cut through with comparative ease by this, and with the very greatest difficulty by the French instrument.

Dr. J. Braxton Hicks, in answer, said that he had used nothing else but the instrument at Guy's Hospital ever since he had constructed it. All of his were made by Durroch, of St. Thomas's-street, and he could say he had never failed except in one case, where the polypus was five inches across, as large as could exist in vagina. The failures, when they occurred, were rather owing to the fault of the makers, who, in imitating the original, either overlooked the principle on which it acted or endeavoured to effect what they called improvements. The most common fault was to leave the eye of the shaft sharp-edged instead of perfectly smooth; this was why the rope snapped over heavy work. Another, which had been alluded to, was its not cutting through in large firm growths. This was owing to twisting the rope too evenly and firmly, which gave it also stiffness. If the strands were rather irregularly twisted, then it would rather cut and tear as one side was drawn down. Another point was the best mode of tempering the wire. Durroch had taken much trouble on this point, and he believed that his make could be relied upon. Dr. Hicks did not intend his instrument to compete in strength with the chain écraseur, although it was nearly as strong; but it was constructed to be used in places where the chain could not be used—at the fundus of the uterus, or on the cervix *in situ*, &c. It was an important point not to use the rope a second time over the same amount of resistance; the cost of a new one was but small. —*Obstetrical Transactions, Vol.* vi, 1865, *p.* 104.

104.—ON OVARIOTOMY AND OVARIOTOMISTS.

By Dr. Charles Clay, late Senior Surgeon of St. Mary's Hospital, Manchester.

[The following brief portion from a longer paper on the subject by Dr. Clay gives the result of his operations. Dr. Clay it must be remembered bore the brunt of the contumely that was for many years levelled against the operation, unsupported and alone. He still considers his plan of a large incision the best, as by it unseen mischief is less liable to be done.]

My operations were begun on the 10th of June, 1842; and I include in the following table all up to the termination of the year 1864, being a period of twenty-two years. And I am anxious to impress on the reader this fact, that all the cases were from private practice, and therefore not so advantageous in many points of view as those from hospital practice.

From 1842 *to December*, 1864.

	Cases.	Recoveries.	Deaths.
Complete ovarian extirpation	111 ...	77 ...	34
Incomplete ovariotomy, cutting down on the tumours, lessening their bulk from within, and setting up the ulcerative process.	4 ...	4 ...	0
Exploratory incisions, discovering uterine tumours, not proceeded with.	2 ...	2 ...	0
Entire removal of large fibroid uteri with both ovaries through the abdominal parietes.	3 ...	1 ...	2
Cæsarean section done too late to save mother, but save children.	2 ...	0 ...	2
Total	122	84	38

Add to the above, six cases of tapping and injection with strong tincture of iodine, four of which never refilled, and, though occurring years ago, remain well. Two refilled, and were afterwards operated upon; both recovered, and are included in the above table. These complete my abdominal surgery for the period.

In the table just given is shown the result of the large incision in every case, except the two exploratory incisions where uterine disease was suspected, and the operation of extirpation not proceeded with. The incision was *in these two cases* smaller, but exceeded four inches, and was as difficult to treat after-

wards as the cases of complete extirpation. I may also remark that these cases occurred in my early practice; and if they were now offered to me in *their then state*, I should not hesitate to extirpate both uterus and ovaries, both being excellent prospective cases. And here I may further state that I have not had the mortification of closing the abdomen over an *ovarian tumour* which I could not extirpate in consequence of extensive adhesions.

In reference to the large incision, the more I have practised it, the more I am convinced of its superiority over every other plan proposed, as the least liable to do unseen mischief, which when once done can never be repaired. Such was the operation of ovariotomy when I first took it up, and the table above shows my position in the question to the end of the year 1864. During the above period, but particularly of later years, many new operators have entered the field, and added their experience. To one and all, when desired, I have answered every question as to mode of operation and treatment. I have kept nothing back that could add to their success.—*Lancet*, *Feb.* 25, 1865, *p.* 202.

105.—ON OVARIOTOMY.

By Dr. W. Tyler Smith, Physician-Accoucheur to St. Mary's Hospital; Examiner in Midwifery in the University of London.

[At a meeting of the Obstetrical Society of London, Dr. Tyler Smith related eight additional cases of Ovariotomy, which with the twelve related in a previous paper, make a total of twenty. His plan is to transfix the pedicle by a double silk ligature, to cut the ends of the latter off and allow the whole to drop into the pelvis.]

Of the twenty cases of ovariotomy now and formerly related to the Society, sixteen have recovered, or 80 per cent. If any one should consider that Case 16, in which the patient died of bronchitis three weeks after the operation, ought to be omitted from the list of recoveries, the cures would still amount to 75 per cent. I have now returned the pedicle into the pelvis in ten cases, and I see no reason to discontinue the practice, but am strongly disposed to recommend it to others. As regards the state of the patients at the time of the operation, I would observe that in all the poor women the disease was so far advanced as to disable them from the performance of their ordinary duties, while in those of better circumstances the condition was such that the operation could not have been delayed longer

without risk to life. All the poor patients were operated on in private lodgings.

Mr. Spencer Wells said that the question raised by Dr. Tyler Smith as to the mode of securing the pedicle was one of the most important which could be considered by the Society. He had followed Dr. Tyler Smith's practice in five cases. From the first no conclusion could be drawn, as the patient would probably have died had the pedicle been secured in any other manner. Two of the other four had recovered more quickly than they probably could have done under any other proceeding, although in both there was more evidence of sthenic peritonitis than was often seen where the clamp was used. In two other cases the patients died of diffuse peritonitis, and he (Mr. Wells) thought they might have done better if the clamp had been used. In one no post-mortem could be made. In the other the ligature and the tissue it enclosed were surrounded by a sort of capsule formed by two coils of small intestine, adhering together and to the pedicle by recent lymph. It was a matter of some doubt whether such a condition might lead eventually to intestinal obstruction, and be more objectionable than the adhesion to the abdominal wall which followed the clamp. His (Mr. Wells') own impression of the three modes of dealing with the pedicle was, that when the pedicle was long the clamp was the safest and the best mode; but that when the pedicle was short, it was better to cut off the ligatures short, and return the tied pedicle, than to leave the ends of the ligature hanging out through the abdominal wall.

Dr. J. Braxton Hicks considered the perfection of ovariotomy would be nearly gained by doing without any ligature at all. He believed it would be possible, by firmly compressing the pedicle for an inch or so by a serrated clamp or some such apparatus, to condense the tissues so firmly as to completely prevent bleeding. The pedicle could then be dropped into the pelvis. He threw out this suggestion because he had found, in examining tissues after the use of the écraseur, that it was nearly impossible to unravel them; and he thought if the width of the compressor were increased no bleeding would result.

In reply, Dr. Tyler Smith stated that he had every reason to be satisfied with the results of the plan of dropping the pedicle into the pelvis after securing it by a ligature, and he could with confidence recommend it as being probably safer and certainly more convenient than the other plan. He should be very glad to have an instrument such as that suggested by Dr. Hicks for dividing the pedicle if it could be relied on for preventing hemorrhage.—*Obstetrical Transactions, Vol.* vi. 1865, *p.* 66.

106.—ON A NEW METHOD OF SECURING THE PEDICLE IN OVARIOTOMY.

By I. Baker Brown, Esq.

[The following article is an abstract of a paper read before the Obstetrical Society of London.]

The Author observed that hitherto there had been three distinct methods of securing the pedicle: first, by ligature, allowing the ends to hang out, as practised by Dr. Clay, of Manchester (the pioneer of ovariotomy in this country, who had steadily led us on to our present successful results), and by Lane (the first surgeon in London who performed this operation) consecutively; secondly, by clamp, as first suggested by Hutchinson, and followed by many others; thirdly, by cutting off the ligature short and closing the wound, as first successfully practised by Rogers, of New York, in 1829, by Dr. Bellinger, in America, in 1835, by Dr. Siebold, of Darmstadt, in 1846, and recently by Dr. Tyler Smith. The first three gentlemen's cases were all successful, and the last-named gentleman has also had great success. Mr. Brown said that his objections to the first method had been the length of time required for the ligature to come away, which varied from nine days in his own practice to a month in that of others; to the second, the frequent severe pain caused by the dragging of the pedicle, or the pressure of the clamp itself; to the third, the unsuccessful results in his hands following its use. Having repeatedly used the actual cautery of late, employing Dr. Clay's instruments, in burning adhesions off the omentum and elsewhere, he had been gradually led to the conclusion that the actual cautery might be employed in treating the pedicle itself. Consequently, on December 28, 1864, he tried it upon a patient of Dr. Burchell, of the Kingsland-road, a lady, forty-seven years of age, who had had three children, the youngest twenty-one years since. The diseased was first discovered by Dr. Burchell in August last, and so rapidly increased as to lead Dr. Barnes and Dr. Tanner to recommend extirpation some short time before he (Mr. Brown) saw her. As the abdomen then was very large, the skin shiny, and the general health rapidly suffering, he performed the operation by Clay's large incision. There were many adhesions laterally and posteriorly, the bleeding from which was checked by the actual cautery; and finally the pedicle, being secured by a clamp, whilst a very large multilocular mass of cysts was removed, was thoroughly seared by actual cautery and allowed to drop. The wound was then closed in the usual way, and it healed in a week, the patient being convalescent in a fortnight. Mr. Brown thought that if this plan was found by repetition to be successful,

it would very materially lessen the dangers of the operation, and consequently ensure a greater number of recoveries.

Dr. Routh stated that Mr. Brown's previous and successful experiments in the removal of the omentum by a red-hot iron would prove the best reply to Dr. Tyler Smith, as to the probable conduct of the peritoneum where a pedicle was removed in the same manner.

Mr. Brown, in reply to several speakers, said that the objection urged by Dr. Tyler Smith, of the slough being injurious to the peritoneum, had been answered by the questions put by Dr. Routh and replied to by Dr. Greenhalgh: that he (Mr. Brown) had, for four years past, repeatedly used the actual cautery in burning adhesions and arresting hemorrhage, and in no one of the cases so treated had he had a death; and he thought that the objection ought not to deter others. He did not allude to the écraseur, because he thought there was not sufficient time in ovariotomy to use it safely; and he did not think it probable that it would ever come into use on that account. In answer to an objection that white heat might be detrimental, Mr. Brown said he did not go quite so far as to use white heat, but he stopped just short of it. To the objection of Dr. Parsons, that there was fear of hemorrhage in case of sickness after the use of the cautery, Mr. Brown replied that whilst it was well known that many patients had died from hemorrhage where the ligature was used, he did not think, judging from his past experience and the results of Veterinary Surgeons in spaying the sow, that there was any probability of hemorrhage where the cautery was steadily and thoroughly applied.—*Medical Times and Gazette, March* 25, 1865, *p.* 320.

107.—ON THE OPERATION OF OVARIOTOMY.

By Thos. Bryant, Esq., Assistant-Surgeon to Guy's Hospital.

[The patient should be in the half-sitting posture as it is comfortable, and still convenient to the operator. There is also less risk of the escape of the ovarian fluid into the peritoneal cavity. In all cases it is better to commence the operation by making a short abdominal incision; but in certain examples, sooner than apply force in the removal of the tumour it is better to increase the length of the wound upwards.]

When the incision has been made, and there is no evidence that adhesions exist between the cyst and the anterior abdominal walls, it appears to be an unnecessary practice, if not one of considerable danger, for the operator to introduce his hand into the abdominal cavity with the view of learning whether

there exist any peritoneal adhesions. If adhesions do exist in front they must necessarily be broken down, but I believe it to be the best practice to separate all others as they manifest their presence during the removal of the tumour, and by following this practice it will be rarely necessary for the operator to introduce his hands into the abdominal cavity at all. When adhesions exist they can frequently be broken down by the finger of the operator, introduced at the wound's margin, for as the cyst empties, these adhesions are necessarily brought forward, and when they do *not* exist the peritoneum escapes even the touch of the hands of the operator. In complicated and exceptional examples of this operation, it is clear that this practice cannot be followed, but in the more ordinary and simple cases it is most applicable. Several of the examples above recorded prove the value of this advice, for in them the peritoneum was touched only by the knife and needle.

Treatment of the pedicle.—How to fix the pedicle with safety and ease, so as to prevent hemorrhage and avoid undue irritation and traction upon the pelvic viscera, is a problem which yet remains for surgeons to decide. The clamp and the ligature are at present indispensable for the carrying out of these purposes; but they are still imperfect, and it is hoped that other means will soon be found which will be free from the objections to the above, and yet possess their advantages. The practice which Dr. Tyler Smith has so boldly introduced, of tying the pedicle and of dropping it with its shortened ligature into the pelvis, is doubtless a step in the right direction; but such treatment has been hitherto applied only to the narrow pedicle, and the treatment of the broad yet remains untouched, although there is no reason why, in these cases, the same practice might not be carried out with an equally good result.

In the examples before us I applied the clamp or ligature according to circumstances. When the pedicle was long, and there was, therefore, no traction upon the uterus, I used the clamp, for its application is easy, and a more rapid convalescence appears to follow its use than that of the ligature. When the pedicle was short I employed the ligature, for it allows the pedicle to retract, and thus has a tendency to prevent that traction upon the parts which must be considered injurious. In other cases, in which the pedicle was very broad, the ligature was used.

Sponging out the pelvis.—The propriety or impropriety of sponging out the pelvis after the operation is a point in practice which experience has not yet definitely decided. Different operators have adopted either method, and apparently with equal success.

In the cases I have related, this practice was not followed,

for in no instance was there any escape of blood of any importance into the peritoneal cavity, nor of any semi-solid ovarian matter, for, under these circumstances, the practice of sponging out the pelvis appears to be a sound one, but under any other it scarcely appears to be required. In a healthy patient, that is, in one who is likely to recover after such an operation as ovariotomy, the effusion of a little blood into the peritoneal cavity cannot be regarded in a very serious light, nor can the escape of the limpid or mucoid contents of an ovarian cyst into the abdomen be looked upon with greater apprehension, for both fluids may and will, in all probability, be absorbed without difficulty; but the irritation of such a foreign body as a sponge upon exposed peritoneum and intestines must be looked upon as being more or less injurious, and, as a consequence, unless a strong necessity compels, the practice of sponging out the pelvis may be considered as being of a questionable nature.

We know from experience that blood may be extensively effused into a joint and no evil result follow, as it may into the cellular tissue of a part, and yet be absorbed; and obstetricians will readily recall cases in which blood has been effused into the peritoneal cavity without, of necessity, any serious result. We gather also, from the above cases, the fact that the contents of an ovarian cyst may escape into the peritoneum by rupture or by a tapping without any evil influence.

In the last case I have recorded, the cyst had evidently been discharging its mucoid contents into the peritoneal cavity for many weeks without the slightest bad effect.

Treatment of the wound.—In every case but one I have employed the silver sutures; but I have *not* done so from the belief that they are less irritating than the silk, for general experience in both has told me that such is not the fact, but because they allow a more accurate adaptation of the parts than do other kinds of suture.

I have also included the peritoneum in my sutures, with the abdominal muscles.

The after-treatment.—The after-treatment of all my cases has been very simple. In nearly all I administered a good opium suppository, as ten grains of the compound soap pill, directly after the operation, and when any pain appeared the suppository was repeated; but in other examples it was administered twice a day, or only at bedtime, according to circumstances. The beneficial effect of this practice in many of the cases was most marked, and in all it was good. It is by far the most effectual way of giving opium in these ovarian operations, as it is in all others involving the intestines, pelvic viscera, or perineum, and in my hands it has almost entirely taken the place of every other mode of administration. By such a practice we appear to secure

all the good effects of the administration of an opiate, without its evil, and at the same time we acquire some additional advantages, for it certainly tends to neutralize the evil influence of chloroform sickness, it gives the stomach rest, and frequently sends the patient off at once into a quiet sleep, the value of which is indisputable.

In the majority of cases this opiate suppository was the only piece of special medical practice requisite, for, in all the above cases which have recovered, convalescence went on from the beginning, and no bad symptoms made their appearance.

The patient's room was kept cool and airy, as in any other case of operation; for the first two or three days beef tea and arrowroot formed the chief diet, but after that fish, meat, and wine, were carefully administered; indeed, the same principles of action were adopted in these cases as have been proved of value in other operations in general surgery; the main object being to keep up the powers of the patient, so as to enable nature to complete the cure.—*Obstetrical Transactions, Vol.* vi. *p.* 56.

108.—ON THE ADMINISTRATION OF CHLOROFORM IN OVARIOTOMY.

By Dr. Parson, York-street, Portman-square.

Since the subject of administration of chloroform has been so recently and so ably discussed by the Committee for that purpose of the Medico-Chirurgical Society, and as the capital operation of ovariotomy is being almost daily performed, the results of my experience in the large number of cases performed by Mr. Spencer Wells at the Samaritan Hospital for Women, where I have had the subsequent care of the patients, as well as in many of Mr. Wells's private cases, may possibly be of some use to others in similar subsequent operations.

The apparatus I have used of late has been "Skinner's," consisting, as most people know, of a piece of domet flannel spread in a tent-like manner over a wire frame, the base of which is somewhat oval, so as to include the nose, mouth, and chin of the patient under this tent-like expansion. The handle is a piece of stout wire about seven inches long, bent into a loop, and can be moved and fixed at any convenient angle with the frame that supports the domet. Hence, the handle balancing the frame, the latter will generally lie easily wherever it is placed on the patient's face. This allows the right hand to hold the bottle of chloroform, enabling the left hand to feel the patient's pulse. The bottle containing chloroform holds about two ounces, and allows the chloroform to escape drop by drop through a very small tube. This is Skinner's inhaler and drop-

ping bottle, slightly modified for the Samaritan Hospital by Weiss and Son.

Its employment is equally simple; the tent-like inhaler is allowed to lie over the lower part of the face, and the chloroform is sprinkled drop by drop over it, and rarely does more than one minim of chloroform at a time escape from the bottle.

The advantage of this mode of giving chloroform appears to be that as only one minim of chloroform escapes at a time, and as the successive minims are distributed over the expanded domet, it ensures a much more regular dilution of the vapour with the air inspired than is usual with almost any other apparatus I have seen, except Clover's, whose methodical dilution is certainly the safest of any, and I know of no one who has presented anything as yet so decidedly accurate as his; but the incumbrance and appearance of the large inflated bag, and the intricacy of the apparatus, will always, I fear, be an obstacle to its general adoption.

Now, the domet inhaler is possibly the next nearest approach to perfection in the simplicity of its use and regular dilution of the vapour, for it follows that if the drops fall regularly, and the inspirations be about the same in frequency and depth, that a regular dilution must be ensured, and hence a nearly equable inhalation, and these I believe to be very important facts for the safety of the patient.

The time usually required to produce anæsthesia thus, varies from ten to fifteen minutes, differing in different persons; but I observe in ovariotomy that the slower its administration the less is the subsequent distress after the operation; that a state of collapse rarely occurs, and the patients generally awake tranquilly and collected soon after the operation; and there is much less vomiting and retching (in no case is solid food allowed before the operation); the preliminary stage of excitement is also very much less; and lastly, there is rarely any spasm of the larynx, which, by the bye, does sometimes occur in hysterical women. The patients generally go quietly into a profound sleep without a struggle. Now, this infrequency of vomiting and retching is of immense importance to the ovariotomist, for vomiting tends to force the small intestines out of the abdomen through the wound, unless the wound be firmly grasped by an experienced assistant, or another assistant presses his hand upon the epigastric region to prevent the descent of the diaphragm, and the operator has to wait till the vomiting is over. And also after the wound is closed by the sutures and strapping, any effort of vomiting is exceedingly painful for the patient, by stretching the abdominal wound, straining the pedicle, and disturbing the small intestines and mesentery that have been more or less irritated by the neces-

sary manipulation in the removal of the tumour and the subsequent sponging and wiping out the cavity of the peritoneum when necessary. Hence, the utmost quietude of the abdominal viscera greatly increases the patient's chance of recovery after ovariotomy.

Cutting through the skin of the abdomen is certainly the most painful part of ovariotomy; next to this appears to be the sponging out of the pelvis; and hence the greatest amount of insensibility is required at the very outset of the operation, and very little chloroform is required whilst the adhesions are being broken down at the time occupied in tapping the cyst or cysts. During the sponging out the pelvis of any fluid that may have collected there, there is almost always a spasmodic action of the legs, which I believe is due to the direct pressure of the sponges over anterior crural nerves, and hold that chloroform should not be given to such an extent as to destroy the irritability of those nerves, and that an assistant should always be placed at the patient's knees to keep them down during this part of the operation. Possibly some of this spasmodic action of the legs may be reflex, and as the patient is unconscious of it, it is much better that those motions of the legs be restrained by an assistant than have the patient prostrated by chloroform. I have also observed that this very sponging the peritoneum has a very depressing effect on the heart's action; frequently the pulse sinks not only in force, but becomes small and slow, In one case I counted it only 30, whereas before it was beating at the rate of 68 in the minute. This woman, aged thirty-two, had a mitral murmur and a large heart, and accompanying this failing of the pulse a death-like sweat broke out over her face; and as I have several times observed this, I fancy the hypogastric plexus has been pressed upon by the sponge, and that this dangerous state is through the medium of the sympathetic; and hence in those cases it becomes the more important not to have the circulation much reduced in power when there is this depressing ordeal to go through.

The chief drawback to chloroform in ovariotomy seems to be that the enormous distension of the abdomen so presses up the diaphragm that the apex of the heart is, I often find, tilted upwards and towards the left axilla—not unfrequently found beating under the third or fourth rib,—and the lungs so compressed upwards and backwards, that the power of making a deep and full inspiration is utterly impossible. I believe the muscular power of the diaphragm to be almost for the time paralysed, comparatively speaking; and hence in almost every case of ovariotomy there is an exceedingly feeble inspiratory power; and as ovarian cases are almost all greatly reduced in muscular power and tone, the state of the respiration during

the operation is also a cause of great anxiety, for I have found that when the patient's mouth is shut, and she breathes solely through the nostrils, that the inspirations are less frequent and less deep than when the mouth is kept open; in fact, in some very feeble old women with ovarian dropsy of some years' standing, one can scarcely see that they inspire at all; and, further, I believe this defective inspiration to be greatly embarrassing to the right side of the heart, for I find that when the inspirations are shallow, the heart beats with less force and less frequency; and in those ovarian operations more attention should be paid to the respiration than even to the pulse; and hence I have of late, when insensibility has been produced, slipped one of my fingers between the patient's teeth, and, throwing her head slightly backwards and to the right side, by this means the tongue does not fall backwards, and the stertor, if it has existed, is generally immediately prevented. I find also that where there is a tendency to spasmodic inspirations, too, and closure of the rima glottidis, it is very effectual in affording a free inspiration.

When the circulation is feeble, notwithstanding that air enters the chest freely, gently rubbing the region over the apex of the heart is sure to increase its action; so, also, is rubbing the neck from the ear to the clavicle over the course of the phrenic and pneumogastric. But I find that this sometimes provokes vomiting: also a cold hand on the hot skin, or a rough, cold towel, has always been sufficient to stimulate the flagging circulation in even protracted operations. Occasionally, when the tumour is very large, immediately the abdomen is emptied the circulation has lost its accustomed pressure, and a temporary fainting occurs. Now this is best avoided by immediately removing one or two of the pillows that have been propping up the patient's head and shoulders, and allowing her to lie perfectly horizontal; and this has to be done whilst the operation is going on, for before removing the mass in the abdomen the patients cannot breathe at all if they are placed horizontally—their diaphragm cannot push down the mass in the abdomen unless their shoulders are raised, and the tumour allowed to gravitate away from the thorax. Mr. Spencer Wells, in a recent case, to avoid the chance of syncope, tapped the patient whilst on the table with a small trocar and canula, and emptied the largest cyst, and thus greatly equalised the pressure before any incision was made into the abdominal parietes.

The average amount of chloroform used is about three drachms. The objection that the chloroform thus administered affects the bystanders I have found to have been of very little importance, and is certainly more a theoretical than a practical objection.

Lastly, I beg to recommend operators *not* to have their patients too profoundly narcotised; for, however pleasant it may be for them, it is highly unpleasant for him who has the life of the patient in his more immediate charge, and I know of no cases so critical as those who are the subjects of ovariotomy. —*Medical Times and Gazette, Feb.* 10, 1865, *p.* 167.

109.—MULTILOCULAR OVARIAN CYST—OVARIOTOMY—RECOVERY.

By Dr. George Scott, Southampton.

[The patient was a married woman, twenty-three years of age, delicate looking, and slightly made. The enlargement had taken place very rapidly.]

When I first saw her the abdomen was already very large, more so than would be that of a woman in the last month of pregnancy. It was equally distended all over, and smooth on the surface, except towards the left hypochondrium, where a tumour of considerable fleshy hardness, of irregularly-oblong shape, and of about the size of an orange, was felt one inch below the false ribs. This swelling was quite painless—it could not be moved out of position, and the abdominal wall could be freely moved over it. She could give no clear account as to when it first became perceptible, but it had certainly been there a month or six weeks before I saw her. The remaining part of the abdomen seemed to be distended only with fluid contained in one large sac, for the wave of succussion was felt passing uninterruptedly from one side of the belly to the other. The liver was pushed up high under the ribs in the right hypochondrium. The line of demarcation between the latter organ and the tumour was not very distinct, the dull, hepatic sound passing almost imperceptibly into the dull percussion sound of the tumour. She experienced a considerable amount of pain in the abdomen now and then, but her general health did not appear to have suffered much since the appearance of the swelling. She could walk about very well, notwithstanding the weight and size of the tumour. Her appetite was very good. Her bowels were remarkably confined—sometimes she had not a motion for a week together. She mentioned that she had all her lifetime been of costive habit. Some years ago her bowels were not moved for nine days, and nevertheless she continued to eat heartily the whole time.

The treatment adopted at this time was the administration of warm water enemata every day, with occasionally a castor-oil one to clear out the bowels, and steel and other tonics internally to keep up the strength. The enemata were continued every

day for some weeks, and brought away for a considerable time an astonishing quantity of hard, offensive fæces.

She was ordered to drink porter, and to have a generous diet, with plenty of meat, &c.

[In December the enlargement had very much increased, and on the 2nd of January two and a quarter gallons of thick, purulent, offensive fluid were drawn off. She was again tapped in March, and the fluid was not so offensive as at the previous time, but her health was evidently breaking down, and the propriety of ovariotomy began to be seriously discussed.]

She was getting thinner and weaker every day,—and the expedient of tapping was certain to be required at shorter and shorter intervals each time it was resorted to as a mere measure of temporary relief. To draw off so much fluid so frequently from the system was a most exhausting process. Moreover, she was leading a life of constant misery, suffering always more or less severe pain in the abdomen. If nothing were done beyond using palliative measures, it was evident that her life could not be prolonged many months, and those would be months of continual and severe suffering. Under these circumstances, therefore, it was pointed out to her that the operation of removing the tumour afforded her a chance of complete recovery to perfect health, although, of course, at the imminent risk of her sinking from the effects of the operation itself within a few days or even hours of its performance.

It was put to herself to decide whether she would remain as she was, using only palliative measures for the relief of urgent symptoms, with a certainty of dying a painful death within a few months at farthest, or whether she would brave the danger of ovariotomy, which afforded her a chance of complete recovery.

She made no difficulty in choosing to run the risk of the operation, observing that she would rather die than continue to suffer the misery she was then enduring. By means of the uterine sound it was ascertained that there were no extensive adhesions of the tumour to the uterus, as the latter was pretty freely moveable all round. It was very much anteverted, being evidently pushed downwards and forwards by the mass of enlarged ovary above it.

On April 27 last, at 3 p.m. I proceeded to perform the operation. I was kindly assisted by Professor Aitken, of the Army Medical School, Mr. Mott, Mr. Norcott, Mr. J. Reeve Shorto, who administered chloroform, Mr. Ware, and several other gentlemen. When the patient was well under the influence of chloroform I made an incision in the middle line about four inches in length, beginning just below the umbilicus, through

the whole thickness of the wall of the abdomen down to the outer surface of the tumour. I then plunged a very large trocar into the large cyst and drew off fully two gallons of thick, viscid, perfectly bland, purulent-looking fluid. Thus having reduced very considerably the size of the tumour, I enlarged the wound in the abdominal wall downwards to the extent of about six or seven inches altogether, and then passed my hand between the tumour and the wall of the abdomen, or rather endeavoured to do so. I found here the adhesions of the tumour so very strong that it was some time, and not without using very considerable force, before I was able to insinuate my hand between the tumour and the abdominal wall so as to commence separating the former from its adhesions. After separating the anterior part of the enlarged ovary, I proceeded to tear away the adhesions from the sides of the same, and then from the posterior part. I was prepared to have to separate a certain amount of connexions to the surrounding parts, but I certainly did not anticipate such firm bands of adhesion and such difficulty in separating them as I actually met with. The whole surface of the tumour was strongly adherent to the surrounding parts, and it was only by using really great force and tearing violently that I succeeded in separating the tumour sufficiently to draw it out of the abdominal cavity. This was, however, done eventually, and the tumour held up bodily over the opening in the abdomen. A loop of the small intestine was then seen to be adherent to the tumour close to its root. This was torn away by the hand without difficulty and the whole mass left attached to the body only by a thin, fibrous pedicle. The clamp was now applied around the pedicle as closely as possible to the tumour, and the latter cut away just above the clamp. The pedicle was sufficiently long to allow the clamp, when drawn out as far as possible without dragging at the parts to which it was attached, to lie outside the wound of the abdomen.

The tumour was of large size. Its weight when removed was 11½ lbs. Fully two gallons of fluid were drawn off before the removal of the more solid part, so that the weight of the whole structure when entire and in the body of the patient must have been upwards of 30 lbs. It was multilocular, being made up entirely of larger and smaller cysts, varying in size from that of a pea to that of the large sac containing fully two gallons of fluid. The contents of the cysts varied very much, being in one sac serous, in another viscid and glutinous, in another purulent-looking, in another very flocculent, and so on. Some cysts of considerable size were growing, as it were, on the outside of the wall of the very large cyst. Several of these smaller sacs, having very thin walls, were ruptured during the operation from the great force required to accomplish the separation of

the surrounding adhesions. The contents of these sacs sometimes serous, sometimes purulent, sometimes glutinous, thus escaped in considerable quantity into the cavity of the peritoneum. This matter, however, being perfectly bland, was not likely to set set up much irritation in the peritoneal cavity. As much as possible of it, and of the small quantity of blood necessarily effused into the peritoneum during the operation was sponged out, and the wound was then closed by means of eight hare-lip needles. A pledget of lint was put over the wound, and a broad bandage round the body so as to cover all, and the patient put to bed. The operation took altogether nearly an hour in its performance; scarcely any hemorrhage occurred, and not a single vessel required to be tied.

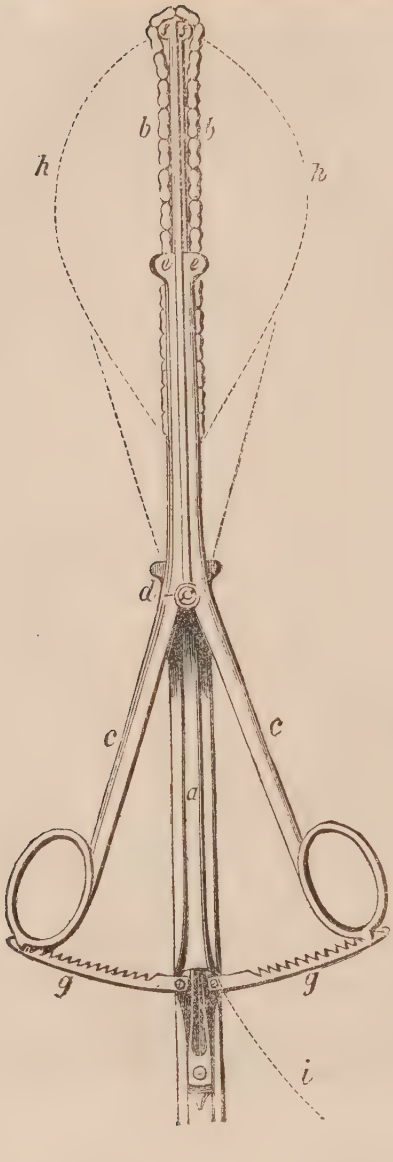
The patient vomited a little after recovering from the effects of the chloroform. Some raw brandy was then given. At bedtime she was ordered a draught containing half a drachm of sol. mur. morphiæ, and a suppository of two grains of extr. opii. was administered. Ice and iced drinks were allowed her freely to allay the thirst; enemata composed of about two ounces of beef-tea with half an ounce of brandy were ordered to be given every two hours during the night.

April 28, 11 a.m. Feels tolerably well; says she is suffering no pain; slept a little during the night; vomited at seven this morning, but not since. Pulse 128. Ordered to swallow small pieces of ice every now and then to relieve the thirst and vomiting. A little beef-tea was given several times during the night, but it was rejected again immediately. After the first night, therefore, for about a week after the operation, nothing but cold drink such as iced-water, toast-and-water, orange-juice and water, &c., was given by the mouth. The patient was kept alive by the beef-tea and brandy enemata continued every two or three hours during the day and most of the night. These were generally well retained. She had a considerable amount of vomiting during the first four days after the operation, but the vomiting seemed to do her good; she always felt relieved by it; there was never much straining with it. 2 p.m. Pulse 120; a little vomiting since 11 a.m.; feels comfortable. Rep. hâc nocte suppositorium extr. opii et haustus sol. mur. morph.—*Med. Times and Gazette, Nov.* 19, 1864, *p.* 536.

110.—ON THE REMOVAL OF UTERINE POLYPI.

By Dr. J. Marion Sims, Surgeon to the Woman's Hospital, New York.

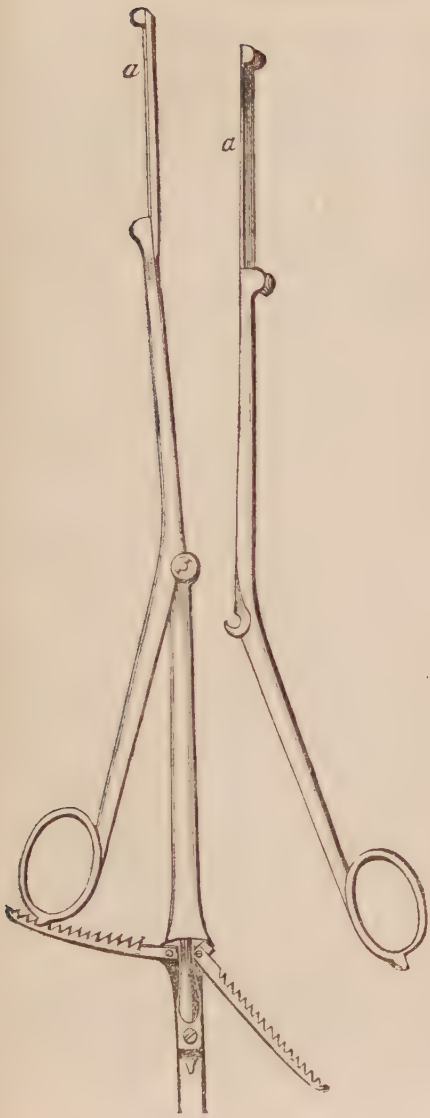
In a former paper (vide the 'Lancet', Oct. 22nd,) I said the écraseur was a most valuable instrument for the removal of uterine polypi; that I preferred Chassaignac's to all modifica-



tions of it; that the chief objection to it was the great difficulty of applying it, and that I had overcome this by rendering the chain rigid. Where the polypus has descended into the vagina Maisonneuve's wire or Dr. Braxton Hick's cord of wire answers very well; but where the polypus is intra-uterine, with a contracted cervix, we ordinarily fail in their application, just as we do with the chain of Chassaignac.

I have added to Chassaignac's instrument a porte-chaine, which may be described as a pair of dilating forceps with spring blades, which render the chain stiff, so that it may be passed straight into the vagina or into the cavity of the uterus, as easily as we would a sound or a sponge probang. After which the chain is expanded by the blades of the forceps or porte-chaine.

Fig. 1 represents the écraseur and porte-chaine united and ready for use. If we have to deal with an intra-uterine polypus we first dilate the



cervix, if need be, with a sponge tent. On its withdrawal the instrument is to be passed into the cavity of the uterus, when the chain, *b b*, is to be expanded in the direction of the elliptical dotted lines, *h h*, by approximating the handles, *cc*, towards the dotted shaft, *a*. The expanded blades, *e e*, *f f*, are fixed at the desired extent by the rackets, *g g*. When the loop of the chain is placed securely around the pedicle of the tumour, the shaft of the instrument is pushed down on the chain just as if there was no porte-chain. Then to get rid of this latter we have only to spring the right racket to the dotted line, *i*, when we pull back the right blade, and immediately the left comes away almost spontaneously.

Fig. 2 represents the porte-chaine separated from the écraseur, with the two blades apart to show their mechanism and the facility

with which they may be detached. The distal extremities of the blades, *a a*, are of watch-spring temper, and have clefts for holding the chain.

Let me illustrate the principle of its application by a clinical observation. In February, 1863, Dr. Morpain, of Paris, invited me to operate on a patient of his who had a polypus as large as a goose's egg projecting partly from the cavity of the uterus.

The patient, on a table, was placed in the left lateral semi-prone position, and when the speculum was introduced, it elevated the perineum and posterior wall of the vagina, and brought completely into view the tumour.

There is great temptation under such circumstances to seize the projecting portion of the polypus with a strong vulsellum or tenaculum, and pull it towards the os externum. But this is not the best thing to do, because it will close up the mouth of the vagina, and obstruct both sight and manipulation; for the mouth of the vagina, even in favourable cases, would hardly be forced open more than an inch and a half from the urethra back to the perineum, and we need all this space for operating.

Here a small tenaculum was hooked into the tumour at *a*, and by it the polypus was pushed gently downwards and forwards against the anterior wall of the vagina. It was held firmly, while the stiffened chain of the écraseur was passed along the upper or posterior surface of the tumour from *a* up to the fundus uteri at *c*. This done the tenaculum was removed, and the chain of the écraseur opened out in the cavity of the uterus to a sufficient extent to allow the tumour to pass through it. This was effected by hooking the tenaculum, and raising the end of the tumour up towards the posterior wall of the vagina, at the same time that the écraseur was pressed in the opposite direction. This movement placed the middle portion of the chain parallel with the anterior face of the tumour, while its loop or distal portion still remained stationary. It was thus made to embrace the pedicle, and it only remained to remove the porte-chaine at the same moment that the shaft of the instrument was pushed down on the chain, which was tightened closely around the pedicle. The operation was then finished as easily as if the tumour had been wholly outside the body, and that, too, without the least strain or traction on the uterus or surrounding organs.

This operation was done with the assistance of Dr. Morpain, Sir Joseph Olliffe, and Dr. W. E. Johnstone. Since then (February, 1863) I have had every reason to feel satisfied with the porte-chaine, whether the polypus was in the uterus or simply in the vagina.

[Dr. Sims then relates a case in which he was consulted on account of sterility. The uterus felt somewhat enlarged and he introduced a sponge tent with the object of ascertaining the cause of the enlargement. This turned out to be a small polypus which was easily removed by the écraseur. Before the use of sponge tents no diagnosis could have been made.]

Now we determine with the minutest accuracy, not only the presence, but the size, position, relations, and attachment of all such tumours. Before the use of sponge tents, if we suspected from rational symptoms an intra-uterine polypus, we could only wait from month to month—sometimes from year to year—for it to grow and to force its way into the vagina, before we could interfere surgically for its removal. But now we no longer doubt and procrastinate; we no longer let our patients bleed till they become bloodless and dropsical: but we ferret out at once the source of mischief, and remove it from its once secure hiding-place. This is a great advance in surgery; and no man of twenty or thirty years' experience can look back on the days of ergot and Gooch's canula, and contrast them with the present time of sponge tents and the écraseur, without a thrill of delight at the progress of our noble calling; and this we owe to British and French surgery—to the sponge tent and the écraseur—to Simpson and to Chassaignac.—*Lancet*, *Nov.* 12, 1864, *p.* 542.

111.—ON THE TREATMENT OF DYSMENORRHŒA.

By Dr. J. Marion Sims, late Surgeon to the Woman's Hospital, New York.

Dr. Simpson's method of incising the os and cervix uteri is followed by most operators, both in my own country and in this. He places his patient on the side, introduces the index finger of one hand into the vagina, pushes the fundus uteri up if it be anteverted, passes his uterotome into the cervix to the os internum, springs the blade, and withdraws the instrument, cutting open one side of the cervix; then reintroducing the instrument, the other side is cut in like manner, thus making a bilateral incision of the cervix large enough to allow the index finger to be passed to the os internum; and, as before stated, he then passes into the vagina a large camel's-hair pencil saturated with a solution of the perchloride of iron.

Dr. Greenhalgh has modified Dr. Simpson's instrument by giving it two blades, which cut through both sides of the cervix at once, thus ensuring an equilateral uniformity of section that cannot always be predicated of the single-bladed instrument. His instrument is a masterpiece of ingenuity, and answers well in his practised hands. But I object to both these methods,

because they are done in the dark, and too much is left to the execution of a machine instead of the judgment of the surgeon.

Suppose it were necessary to amputate an elongated uvula,—by no means an uncommon operation,—would it be judicious to run one finger down the throat and guide by it some machine for performing the operation in the dark? Or would it be more surgical and more precise to look into the throat, seize the part with a proper appliance, and amputate it where our judgment would determine to be right and best for the individual case? There are operations that must be done by the touch alone, and in the dark; but we never select this plan if it be possible to aid the manipulatory process by the sight.

Besides the objections already urged against instruments of this class, there is another to which all instruments on the principle of cutting from above downwards are obnoxious—viz., that as the uterus is not fixed, it must necessarily glide upwards to some extent by the mere centrifugal force of the expanded blade or blades, and thus we can never feel altogether certain of the length and breadth of the cut. Whether too much or too little, it is not safely remediable afterwards.

FIG. 1.

The above engraving represents the speculum elevating the posterior wall of the vagina, the tenaculum hooked into the anterior lip of the os tincæ, which has here been divided on one side. The scissors are seen in the act of dividing the opposite side.

The operation, as I prefer to perform it, differs from Dr. Simpson's, not in its aim and scope, but merely in its mechanical

execution. He and his followers operate in the dark. I bring everything into plain view. They cut from within outwards; I, in the contrary direction, from the os externum upwards to the cavity of the womb. They, as a rule, do not tampon the vagina after the operation; I always do, for the double purpose of guarding against hemorrhage and ensuring an open os.

I place the patient on the left side, as for all the operations in uterine surgery. The speculum is introduced; a small tenaculum is hooked into the central portion of the anterior lip of the os tincæ; the uterus is gently pulled forwards; one blade of a pair of curved scissors is passed into the canal of the cervix till the outer one comes almost in contact with the insertion of the vagina on the side of the cervix, and the portion thus embraced is divided at one blow of the scissors. Then the opposite side is in like manner divided, and the operation is almost finished. (Fig. 1.) It only remains, while the uterus is still held in position by the tenaculum, to sponge away the blood, and pass a narrow-bladed, blunt-pointed knife (at a proper angle with its handle) and divide the small amount of tissue on each side leading from the scissor-cuts up to the very cavity of the womb.

Fig. 2 represents the knife with the blade in proper position for cutting the left side of the canal. To cut the right side it is necessary to turn the blade in the opposite direction, as shown by the dotted line. The blade may be fixed firmly at any angle by the screw at the end of the handle, which drives a shaft up into little holes, as seen in Fig. 3, where the razor-shape of the blade is also shown. The operation is quickly done, and the judgment of the surgeon determines whether the peculiarities of the case demand more or less cutting. The hemorrhage is usually unimportant, but sometimes it is furious, and I have often seen it come with such a rush that the vagina would be filled before a set of sponges could be washed out. But there is nothing to be feared. Press one or two sponge probangs (Fig. 4) right into the neck of the uterus, but at the same time be sure to keep the organ firmly fixed by the tenaculum; for if the bleeding is profuse it is a very awkward and unlucky thing to let it slip out, particularly if the vagina be lax and deep. Five or six minutes will usually suffice to control the bleeding by the pressure of the probangs. When that is done, and not till then, the dressing may be proceeded with. Two or three small pieces of cotton, large enough when moistened to fill up the gaping os, are to be thoroughly saturated with water, then squeezed as dry as possible, and afterwards wetted in a mixture of one part of Dekau's neutral solution of the perchloride of iron with four or five parts of water, or in Dr. Squibb's liq. ferri persulphatis similarly diluted. Squeeze

out the superfluous fluid, and place a bit of the cotton in one angle of the wound, pressing a portion of it up into the cervical canal, and holding it in place with the sponge probang. Apply

Fig. 2.

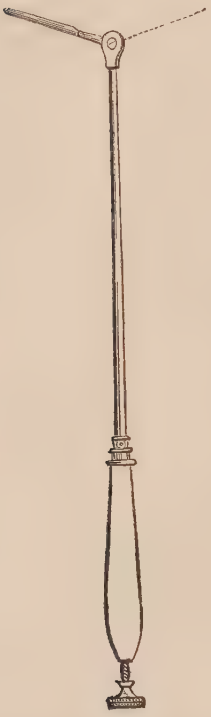


Fig. 3.

Fig. 4.

another bit of cotton similarly prepared on the opposite side, and press it down with another sponge probang. If necessary, another portion of cotton may be placed centrally or over all; then, if there is no bleeding some cotton a little moistened in simple water may be laid over the neck of the womb, to be covered with dry cotton to the extent of supporting the whole dressing neatly and comfortably in its place. The patient is put to bed, having been perhaps eight or nine minutes on the table. She eats and drinks as usual, but the recumbent

posture is enjoined for a few days. She may pass water lying, or it may be drawn off. The only object of the recumbent posture is to ensure the retention of the dressing *in situ*. I formerly allowed my patients to sit up and walk about the room, the day after the operation, but I was so often annoyed by the supervention of hemorrhage that I at length adopted the plan of keeping them down till the spontaneous separation of the intra-cervical dressing.

On the day after the operation the whole of the vaginal portion of the tampon is to be carefully removed, placing the patient in the position as for the operation, and using the speculum, which must be introduced so as not to derange the relations of the dressing. When it is all removed down to the intra-cervical portion, a wad of cotton saturated with Price's glycerine, and large enough to cover completely the cervix and its first dressing, is laid over it, and the patient again lifted into bed. The action of this, as already fully explained, is to induce a profuse watery discharge from the vagina, which keeps the part cleanly drained of all secretions or exudations from the decomposition of the blood contained in the original dressing. This glycerined cotton is to be removed and renewed daily till the suppurative process throws off the dressing from the neck of the womb. This will not be under three or four days. In the meantime, the glycerine, by its detergent and antiseptic properties, keeps everything sweet and clean; and its affinity for water, which by osmosis it extracts from the tissues with which it lies in contact, keeps the parts entirely clear of any secretion that might be re-absorbed and poison the blood, if not thus drained off by the chemico-capillary action of the dressing. No one can thus apply glycerine to the neck of the womb and not be struck with its peculiar power and properties. The intra-cervical dressing will be loosened on the third day or later, and it may then be gently removed with forceps. If it adheres obstinately, let it alone, but cover it and the whole cervix with the cotton glycerole, and at the next dressing it may come away easily. I have frequently provoked bleeding by a little impatience in removing it prematurely. When it is once safely out, then the cervix is to be plugged with a small bit of cotton glycerole, and the whole covered as before with the same. This dressing is to be renewed daily till the parts have entirely healed, which usually takes from twelve to seventeen days, or perhaps till the recurrence of the next menstrual period. And this reminds me that the operation should always be performed within from three to five days after a menstrual epoch, so that we may have time enough for the healing process to be wholly accomplished before the recurrence of the next.

There is sometimes great trouble in keeping the mouth of the

womb sufficiently open. It never remains just as we cut it. The tendency of all cicatrizing wounds to contract as they heal is wonderfully illustrated here. I have often been amazed to find the os contracted in a month to one-fourth of the size of the original incisions. I have frequently seen it cut open large enough to admit the index-finger up to the os internum, and then close in a few weeks to such a degree as not to admit a No. 4 or 5 bougie, and this in spite of persevering efforts to prevent the contraction. This is the case where there is great induration of the cervix, with deposit of white fibrous tissue. I have frequently been compelled to repeat the operation, and I remember several patients upon whom I have operated as often as three times in the course of a few months, and even then the result was not wholly satisfactory. These may be called exceptional cases, but it is well to know that they are not very rare. Even when the os tincæ remains open enough, we may have some trouble in keeping the contracted portion above of normal dimensions. This may be the case if there is much of a flexure, particularly anteriorly. And here I would recommend the occasional passage of a bougie after the first week. Dr. Emmet is in the habit of using the sound as early as the third day after the operation, passing it into the cavity of the womb, and pressing it pretty firmly first against one side of the canal and then against the other in withdrawing it. I have in a few cases followed his example, but with a little timidity. Dr. Greenhalgh uses a self-retaining intra-uterine stem, which is very ingenious, and answers well in his hands.

This operation often cures dysmenorrhœa. Sometimes it only modifies it. And again, I have seen cases where it produced no beneficial effect whatever. The first menstrual flow after it is usually ushered in without the premonition that had so long harassed the poor sufferer, and she may pass through the whole period with comparative comfort; but I think it advisable for such patients to take very good care of themselves at each return of the flow, and to avoid all unnecessary exposure or fatigue. If there is pain enough to lie down, I direct an anodyne by the rectum, and for this purpose McMunn's elixir of opium is the very best. It is less apt to nauseate or to produce headache than crude opium or any of its alkaloids. It is more efficacious by the rectum than by the mouth, because it is more immediately in conjunction with the nerves of the affected part.

Morphine taken into the stomach is a very poor remedy for sciatica, but we all know that it exerts not only a temporary soothing effect, but a wonderful and rapidly curative action when endermically injected along the course of the affected nerve—a boon to suffering humanity for which we owe an ever-

lasting debt of gratitude to the honoured name of Alexander Wood, of Edinburgh. It is upon this principle of applying the remedy as near as possible to the seat of suffering that the anodyne for dysmenorrhœa is far more efficacious by the rectum than by the mouth.—*Lancet*, *April* 1, 1865, *p.* 338.

112.—ON DILATATION VERSUS DIVISION OF THE CERVIX UTERI IN DYSMENORRHŒA.

By Dr. G. T. Gream, Upper Brook-street.

[Dr. Marion Sims as will be seen by the preceding paper repudiates dilatation of the os uteri as dangerous in all its aspects, and declares that division of the cervix is as safe as dilatation is hazardous. This opinion Dr. Gream contests.]

It is now many years since this operation was proposed by certain practitioners as a cure for dysmenorrhœa and sterility; and from that time I have repeatedly been consulted by women who, at different dates antecedent to my seeing them, had undergone the operation; and, except in one solitary instance, I have met with no one in whom pregnancy had supervened upon upon this mode of treatment; indeed, their application to me was consequent upon the failure of the plan which had been adopted.

The exceptional case was this. A lady to whom the birth of an heir was most important found herself with child, and sought my attendance when she had arrived at about the fourth month of pregnancy; telling me—without, however, attributing her pregnancy to the operation—that the uterus had six years before been operated upon; and so complete had been the division of the cervix that the finger could readily be introduced into the uterine cavity, and the membranes of the ovum could be touched, as they can be sometimes during the last days of gestation. The patient was an old friend of a physician whose name it is only necessary to mention to give assurance of his ability to judge of these matters; and I begged that his opinion might be asked in reference to the chance of a continuance of the pregnancy. His opinion corresponded with my own, and, according to our prognostications, abortion resulted but a few weeks afterwards, from the inability of the uterus to retain its contents, I need not say that the hopes of this patient are irreparably gone.

I could relate cases of cellulitis, of pelvic abscess, and of chronic as well as of acute metritis, depending entirely upon the performance of this operation; but the following case will show some of its ill effects. A lady arrived in London in a deplorable condition, the effect of an operation upon her

uterus which had some time before been performed. She was most desirous to reach home, and had been permitted to do so only upon her urgent entreaties, because her general condition was such as to give alarm lest she might not accomplish her journey. The physician who had been attending her, whose advice was sought after the operation had been performed, and when its bad effects became apparent, wrote to me, begging me to give her my most serious attention. It was desirable that she should be often seen; and Mr. Morgan, of Sussex-place, kindly undertook the charge of her, living as he did near to her own residence; and he and I conjointly attended her for months during the formation of an extensive pelvic abscess, which ultimately discharged itself into the intestine, and she recovered, but with this result: her uterus is permanently fixed to the left side of the pelvic cavity, and any chance of future conception is totally at an end.

I could relate cases of alarming hemorrhage immediately following division of the cervix, and others in which the lamentable consequences were deferred, and in which death ensued; and I confess I was surprised to find that Dr. Marion Sims had been assured by some of the promoters of this plan of treatment that they had never met with hemorrhage; for I fancied that my memory carried me back to evidence of a contrary nature. But of this I will say no more.

The treatment of sterility is but seldom called for, except by those in the more wealthy classes of society, as the birth of children to persons amongst other classes is comparatively of less importance; and hence experience is gained in mature private practice rather than in hospitals. It seems incumbent on those, therefore, who have had the opportunity of treating cases of this kind, to make public their experience; and I am sure that those of your readers to whom I am known will give me credit for stating the truth, when I say that there are numbers of women in the upper classes of society who would sacrifice half their lives to become mothers, but who, from having undergone the operation of division of the cervix uteri, have lost all chance of being pregnant.

I agree with Dr. Marion Sims that dilatation may prove hurtful, indeed may be dangerous to life; but not when properly performed. I have myself seen the uterus enlarged to the size of the fifth month of pregnancy by the undue means taken to dilate the os. I have known instruments so rudely forced through the canal that acute symptoms have followed, requiring most active means for their suppression. And I have myself found pieces of metal fixed in the uterus, which had remained there, in some instances, for days, and in others for longer periods; while inflammation was set up, and abscess

resulted. I have known repeated dilatations followed by chronic as well as by acute inflammation; and this in single women. In one case—a case ultimately in charge of Mr. Turner, of Sussex-gardens, who asked me to meet him in consultation concerning it,—there had been no less than sixty instrumental introductions with the view to remove dysmenorrhœa; and this in an unmarried woman. But these are not the cases exemplifying mischief by dilatation; for common experience shows that the uterus will not admit of the permanent introduction of foreign bodies without bad results, and prolonged repetition of dilatation is in no case called for. I entirely discard, therefore, all such cases as arguments against dilatation, as well as others mentioned by Dr. Marion Sims; and I repudiate, with all the power that words possess, the tampering with the virgin uterus under any but the most urgent suffering.

I remember when I was a pupil of Sir Benjamin Brodie, at St. George's Hospital, that great surgeon introduced the practice, when extracting a calculus from the female bladder, of dividing in two or three places the mucous membrane of the urethra, throughout the whole canal, by means of a bistouri caché, in order that, as cicatrization proceeded in these wounds, the urethra might contract, and thus prevent the incontinence of urine which usually followed in such cases.

Now it appears to me that there is no difference in the behaviour under cicatrization between the mucous membrane of the cervix uteri and that of the urethra. Hence this operation becomes rather an impediment to than a cause of pregnancy. And when it is remembered that we are acting upon the most dilatable part of the whole body—so dilatable, indeed, that the fœtal head even escapes through it by dilatation,—why should we seek other methods for enlarging the os uteri than dilatation, when it alone is the process especially pointed out as fitted to the nature of the part under treatment?

I regret exceedingly to differ from Dr. Marion Sims; but when I have been witness for many years past to the cure of dysmenorrhœa in married women through dilatation of the os uteri, and when I have seen pregnancy repeatedly and often immediately supervene upon the same operation, it has occurred to me that I have some claim to your indulgence and that of your readers for considering the two questions of incision and dilatation in an opposite point of view to that entertained by Dr. Marion Sims.

It is far from my wish to be supposed to suggest to so experienced a practitioner as Dr. Marion Sims appears to be the adoption of a mode of treatment which differs from his own; but he has evidently taken his objections to dilatation of the os

uteri from cases which in my view were evidences, not of dilatation, but of maltreatment of the uterus. If, however, he will try the experiment of introducing a very small flexible metal bougie, such as is used for the male urethra, about two inches into the os uteri, and follow this by others gradually increased in size until the canal is dilated to the size of a common pencil; or if he will use a dilator such as was mentioned by Dr. Priestley, I think, in the 'Lancet' a few months ago, or one which was made for myself, and which was described by me in your journal,—he will find it more successful in cases of sterility than slitting the cervix uteri, and thus inflicting a permanent injury on that organ.

The whole process of dilatation may be performed in two or three operations at the most.—*Lancet, April* 8, 1865, *p.* 381.

113.—A SIMPLE INSTRUMENT FOR VAGINAL INJECTIONS.

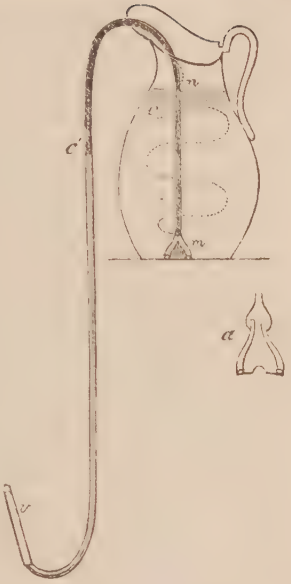
By Dr. Adolph Rasch, Physician to the Eastern Dispensary of the German Hospital.

The instrument I have the honour of bringing before the Society consists of a long india-rubber tube (six to seven feet long, and about as thick as a lady's little finger), made heavy at one end, with a hollow, bell-shaped lead, the base of which has large grooves, to allow the entrance of the fluid from the very bottom of the vessel. It is essential that the india-rubber tube be strong and well made, to prevent its collapse. An elastic thick bougie, with several holes in the bulbous end, and not less than five inches long, is fastened at the other end of the tube.

To use this instrument, place some vessel on a high place (mantlepiece, chest of drawers, &c.), plunge the metal end of the instrument along with about two feet of the tube into the fluid to be injected (the dotted lines show the tube in this stage), and allow the immersed part to fill, which is done in a few seconds; then compress the tube with two fingers over the surface of the water, and lift the *filled* part of the tube over the brim of the vessel and *lower* than the surface of the fluid in the vessel. By this little trick we establish a current in the syphon, and make all contrivances for suction, &c., unnecessary.

The drawing needs little explanation. By straightening the filled tube, *c* will be at *c'*, that is to say, lower than the surface of the fluid, *n*. The dark part shows the tube filled before the compressing fingers have been withdrawn. *a* Represents the metal (the under surface of which is grooved) to which is attached the long flexible tube.

The fluid will, of course, be propelled out of the vaginal tube the more forcibly the higher the vessel with the fluid is placed. When using the instrument the patient should sit over a basin to receive the fluid from the vagina.



Greatly as I rely on injections in the treatment of diseases of the vagina and uterus, I have considered it a duty to bring my simple contrivance before the Society. Having no complicated construction, every one can put it together himself. It cannot get out of order, and deserves a trial.

As the quantity of fluid for a douche should be large, it is advisable to order the ingredients in powders or concentrated solutions to be mixed with a jugful of water. As alum, zinc, lead, &c., are very cheap, there can be no objection to this, even in the practice among the poor.

The instrument is made by Mr. Evan Lewis, 31, City Road. —*Obstetrical Transactions*, *Vol.* vi, 1865, *p.* 224.

114.—ON THE SURGICAL TREATMENT OF VESICO-VAGINAL FISTULA.

By Dr. Awly P. Banon, Member of Council of Royal College of Surgeons, Ireland; Surgeon to Jervis-street Hospital, &c.

[In the first three or four cases operated on by Dr. Banon he followed Bozman's plan, and with success. After shortly describing this operation, he passes on to observe :]

For very small fistulæ, particularly when situated high up in the vagina, I have myself contrived an instrument which I shall endeavour to describe, and which I found of much use in fixing and bringing within easy reach of the knife the edges of the aperture. The instrument is a curved steel sound or probe,

provided with four elongated points projecting backwards. It may easily be forced into the smallest aperture, and on withdrawing it will grapple the margin of the fistula on every side, and enable the operator to divide it with the greatest ease. Its introduction will be much facilitated by previously passing a sound through the urethra into the bladder, by which the membrane can be made tense and raised towards the operator. The necessity of some such instrument suggested itself to me in August, 1863, in the case of Mrs. Quanby, which is one of those I am about to detail. In her case the vagina was contracted, and the fistula, a mere pin-hole, situated very high up, the instrument proving of much use in bringing it within reach. I subsequently used it, and assisted Drs. Kidd and Cronyn, who recently operated in cases of this form of fistula, and found it greatly to facilitate their operations. In my earlier operations, after pairing the edges and introducing iron or silver wire sutures, I followed the plan of Bozman, of using leaden plate perforated with a double row of holes corresponding to the number of sutures intended to be used, and cut to the proper size, on which the sutures were twisted; but though I found it to answer well, I have latterly omitted it as unnecessary, and have followed the more simple plan of Marion Sims, of twisting the silver sutures down on the divided parts, having, of course, previously brought them accurately into apposition. I had the advantage of seeing Dr. Sims operate when in Dublin, two years since, and had several conversations with him, and was so struck with the simplicity of his method that I have since then, to a great extent, followed it. The position recommended by Sims is the semi-prone, which is much less harrassing to the patient as well as to the operator than that on the hands and knees recommended by Bozman. This latter will, however, in some cases be found convenient, as will also that of the lithotomy position recommended by Baker Browne. Chloroform may be advantageously used in the semi-prone or lithotomy positions, but not in that resting on the hands and knees. After the operation the patient should be kept quiet, and the bent or syphon catheter introduced and left in the bladder, changing it every day, or oftener if required, until the 8th or 9th day, when the sutures may be removed, and the patient allowed to pass urine by her own efforts. I am aware that some surgeons do not consider this course necessary, and dispense with the catheter altogether. Of the results of this practice I have no experience, having never tried it, but knowing that all strain must be kept off from the recently divided parts by the retention of the bent catheter in the bladder, and that it is generally borne without much discomfort, I should be slow in giving up its use.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 30.

115.—CHILD CROWING—THE MOST EFFECTIVE REMEDY EXPOSURE TO DRY, COLD WINDS.

By J. Roberton, Esq., Manchester.

It may be said, and with truth, that various authors among their remedies for this disease recommend change of air. It is not, however, mere change of locality that I advise, but the free exposure of the infant out of doors, for many hours daily, to a dry, cold, atmosphere; and if the air be dry, the colder the better. The following, from the pen of the late Dr. Marshall Hall, is worthy of attention; and I refer to his opinion regarding atmospheric exposure in child-crowing on account of his acknowledged eminence both as a writer and a practical physician:—

"The baneful influence," says he, "of the north-east wind, and the curative influence of change of air, and especially of the sea breezes, are not less marked in this affection than in whooping-cough." Again: "All inclemencies of the weather being avoided—for heat, cold, and the north-easterly winds are alike injurious—the child should be much in the open air."

The five following cases, selected out of a number in which I have been consulted since the publication of my paper several years ago, and all of which have been treated on the same principle, are instructive, especially as showing, contrary to Dr. Marshall Hall's opinion, that easterly and northerly winds are not injurious but curative. Were these winds injurious it would indeed be unfortunate, since, owing to causes I cannot account for, every instance of child-crowing that I have seen has commenced in the winter or spring, as the following list of cases (of which I possess notes) proves:—In November there was one, in December four, January seven, February eleven, March thirteen, and in April four; in all forty cases.

In the way of preparation for change of air the following particulars demand attention:—

1st. Careful scarification of the gums, so as to make and keep bare the tips of the advancing teeth.

2nd. Correction of the alvine secretions, which are nearly always of a pale colour, by small doses of mercury with chalk shaken up in a little syrup.

3rd. Free sponging of the body every morning with cold water.

4th. Warm clothing, suitable to the season of the year.

5th. If the child is in part or altogether handfed, careful attention to the diet; since excess of food is found to aggravate the crowing and other symptoms.

Case 1.—January, 1856.—E. H., a girl twelve months old, with only two lower incisors cut, and suffering from the gums

over the upper incisors, and crowing spasms very frequently, and while in them was stiff and struggled severely. The thumbs were turned into the palms of the hands, and there was blueness always remaining under the eyes and round the mouth and nose. Was taken in January to Alderley, but the weather being damp and close, she grew rapidly worse while there, having as many as thirty-two attacks of crowing in the twenty-four hours. At the end of a week she was brought back to Manchester, and then taken without delay to the shore below Liverpool. There the weather was found to be intensely cold, and a strong and bitter east wind blowing. Sometimes the nurse who carried baby about could hardly stand with her in her arms, so strong was the wind. Baby was a month at the shore, being out in all weathers for about six or seven hours a-day. From the end of the first week there was evident improvement, and by the end of the third week the crowing spasms had ceased, nor has she ever suffered from a return of them. She was sponged with cold water night and morning; had careful attention paid to the diet, especially as to the quantity she ate; and before the two upper front teeth (which caused her much suffering) came through, her gums had been lanced eleven times. She is now a strong healthy child.

Case 2.—February, 1865.—H. P., a boy ten months old, had for some weeks been feverish and starting in consequence of cutting his side teeth; the gums were frequently scarified, giving relief at the time. About a week before "crowing" was heard he had two convulsions, and for some days was seriously ill. "One morning," as the mother wrote me, "on going up early to his room the nurse drew my attention to his hands—the thumbs were drawn into the palms; and when dressing him I found his feet were also drawn, so much so that I was unable to put on his boots. The nurse told me she had been alarmed by the child making a peculiar noise as though struggling for breath. I remarked also a blueness about the mouth and nose, which made me fear he had been convulsed during the night. I had heard you describe the symptoms of "crowing inspiration," and therefore lost no time in sending for you. Your order was to take him off without delay to Blackpool or Fleetwood. On account, however, of the severity of the weather, and his having a cough, I sent him with his nurse to Southport. In a day or two I went to see him, and found him very little better: his hands and feet were not so much drawn, but the "crowing" was more frequent. On my reporting this, you considered Southport not sufficiently bracing, and you wished me to take him to the bleak shore at Fleetwood, which I accordingly did. There had been a heavy fall of snow, and a north-east wind prevailed more or less the

whole time at Fleetwood. Every day, wet or fine, he was wrapped up warmly and drawn out in a perambulator from morning till evening. He improved daily; the fits of "crowing" became gradually fewer and less severe, and at the end of three weeks he returned home perfectly well.

Case 3.—January 11, 1857.—Visited Mr. W.'s baby, a male, aged fourteen months, suffering from the incisor teeth, and from fits of crowing with clenching of the thumbs. The gums were scarified, the bowels regulated, care enjoined as to diet, and it was intimated that he must be removed immediately to the sea side. In about a couple of days an attack of crowing terminated in convulsions, and as the child's grandfather happened to live near Disley, at the foot of the hills, the family, with my approval, had him carried thither. On reaching Disley, as the nurse was lifting him from the carriage, he had crowing followed by convulsions. The weather was dry, the cold intense, and the country deep under snow; nevertheless, he was carried daily to the top of a neighbouring hill, and afterwards in a perambulator was wheeled about in the snow the most of the day, greatly to the surprise of the country people, who thought the nurse must be out of her mind. A man said, "Are ye mad, lass? ye'll kill that child." She replied, "The doctor has ordered him to be carried on the hills." "I don't care about Doctors," said he, "ye'll kill him." The crowing was rapidly mitigated, and I was assured by the babe's aunt, who accompanied the nurse, that the crowing and other symptoms had disappeared by the end of a week.

Case 4.—February 4, 1857.—Was consulted regarding a fine, vigorous, male infant, aged eleven months, suffering from the incisor and biscuspid teeth, with crowing and drawing of the thumbs. After dividing the gums, minding the bowels, the diet, and the clothing, I advised a removal to the sea side. The baby's mother, however, having occasion to visit a relative, about ten or twelve miles north of this, in the country, took him thither for change, but with little or no benefit, and on her return, the crowing being frequent and of alarming violence, I advised his being taken forthwith to the dry breezy shore of Blackpool. His mother wrote me from Ulverston as follows:—"We have come here after a month at Blackpool, and I am thankful to be able to give you a satisfactory report. You will remember that the day we last brought baby to you his thumbs were in; we went that afternoon to Blackpool, and before night the thumbs were out, and have only been in once since for part of a day, which was about the tenth day after we got there. We were not able to carry out your system fully for the first ten days, on account of a sharp cough, the weather being at the time most severe, but as soon as the cough mode-

rated we had him out all weathers—for the first week from five to seven hours daily, the second week from seven to eight hours, and for the last week he has been out between ten and eleven hours each day. We took him out both when sleeping and waking, in hard frost, in the bitterest east wind, in a north westerly wind that I could scarcely stand against with him in my arms, in hail-storms and in snow, and yet he never suffered from cold, but on the contrary benefited visibly in health, whilst the crowing gradually diminished. We wrapt him up well certainly, and in that inclement weather he was the only one of the party to come in warm. I have strictly followed your directions; the diet is plain, and the meals given with regularity; the bowels are in order, and he ails nothing but the occasional crowing, which has now ceased to be alarming. His eye tooth and four double teeth are coming, yet it is wonderful how little they affect his health and spirits; he has twelve teeth through, and is now 13½ months old." On returning from Ulverston after two or three weeks, the crowing and the other symptoms had ceased.

[Mr. Roberton, after relating the fifth case, which was of a similar character, says:]

I have, in conclusion, to observe that in the numerous cases of stridulous crowing that have come under my notice, painful dentition has been the sole cause. I have never seen or heard of one instance of the disease before or after dentition—always during the teething period; and the opinion advanced by some that child-crowing is owing to morbid enlargement of the glands of the neck, or of the thoracic absorbent glands, has no support from facts.—*Medical Times and Gazette, Jan.* 14, 1865, *p.* 32.

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